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The Village in an Industrial World*

RALPH L. BEALS

The author is a native Californian and was educated at the University of California. He received his Ph.D. degree from that university in 1930 and is now chairman of the Department of Anthropology and Sociology, the University of California at Los Angeles. He was honorary professor, Instituto Politecnico, Mexico, 1939, and a member of the Faculty of Medicine, University of Concepción, Chile, 1948. In addition to his university work, Dr. Beals has done extensive editorial work and has been continuously active in many anthropological groups. His principal fields of work include California, Mexico, Arizona, and many South American countries.

LONG experience suggests that an anthropologist must explain himself more fully than must the chemist or the psychologist. For years one of my colleagues has believed my greatest interest to be the collection of arrow heads, while another has tried unsuccessfully to present me with a fossil elephant. Some of my colleagues have wondered how I could bear to live among a lot of dirty natives, while others have envied my opportunities to live among noble and unspoiled savages. Actually, the varied and superficially confusing activities of anthropologists have a very simple common theme. Anthropologists are interested in people, in their past, their present, and their uncertain future.

An interest in people explains the first part of my title, for most people are farmers living in villages. Statistics are poor or nonexistent for many parts of the world, but by the most conservative estimate more than two-thirds of the world's people live in villages; the figure may well be seventy-five or eighty per cent or more. Moreover, the village is a conveniently small unit for the study of many problems of theoretical importance.

Numbers and convenience are not the only reasons to study the village. Today the village world is in a ferment of change under the mounting impact of industrial civilization, and gives rise to some of the most critical problems of our times. The varied conditions of change likewise provide as near an approach to laboratory conditions as the student of man is likely to find. Both the needs of science and urgent practical affairs call for study of the village. It is in this framework that I review my own research in the villages of Latin America.

Whether it be in Latin America, Asia, Africa,

* The field work upon which this paper is based was supported by grants from the Social Science Research Council, the Wenner-Gren Foundation for Anthropological Research, and the Committee on Research of the University of California, Los Angeles.

or elsewhere, village communities untouched by the industrial world tend to have common characteristics in contrast with the city. Most economic life is organized about the production of food. Specialization of labor tends to be by age and sex, and within each family is to be found knowledge of the basic techniques to produce the minimal necessities for food and shelter.

The members of the village community are known to one another. Social relationships tend to be on a face-to-face basis. Chiefs or headmen often are mediators rather than figures of authority. Although conflicts and methods of adjudicating them are always found, social controls tend to be indirect and informal. There is less need for a judge when everyone knows the rules. Social status and prestige are related to family size and standing and individual abilities. The social system tends to be limited to the village, even though integrated into some larger system governing relationships between villages. Religion or ritual affects most aspects of culture. Even if specialists are found, each family head is on occasion his own priest.

The village world tends to be stable in its social structure and adequate in fulfilling its cultural functions. It maintains its membership and satisfies in tolerable measure its basic physiological needs. Both birth rates and death rates are high, but the population tends to be in balance with resources and technology. Demands upon the individual for adjustment are limited to changes in age and status. Child-rearing practices tend to prepare individuals for adult life and to shape personalities to fit the existing social and cultural demands. The way of life appears to the individuals in it, and conditioned by it, to be rational, stable, satisfying; for them it is a proper way of life.

The urban way of life is very different, especially now that industrialism creates ever broader demands for markets and raw materials. Economic life involves far more than the production of food and raw materials. The family no longer is the economic unit, and shrinks toward the parents and dependent children. Labor grows increasingly more specialized and dependent on complex organization. No man can know or comprehend the sources of all the economic goods for which an industrialized society has created secondary needs; men grow increasingly interdependent.

Face-to-face social relationships of importance are no longer with relatives and neighbors, but with chance associates or in the many voluntary associations. Formality and impersonality mark social intercourse, just as formal controls, police, and judiciary replace the more informal machinery of village justice. When no man knows his neighbor, he tries to make him take an oath.

On reviewing my own research in Latin America in retrospect, it is increasingly evident to me that its central problem is the impingement of the urban industrial world upon the village. Among the Yaqui and Mayo of Sonora, the sacred clowns perform traditional antics in the midst of machines. Tractors and irrigation systems compete in interest with the insistent demands of a complex and interdependent social and religious system. In some Tarascan villages in central Mexico half the adult males have worked in the United States, and highways knit once remote villages to urban centers. Among the isolated Mixe of southern Mexico, schools and sewing machines compete for attention, while pre-Columbian rituals that survived centuries of missionary hostility are quietly disappearing. Only one in five Mazatecs speaks Spanish, but in their midst is taking place one of the greatest river development projects in the Western hemisphere.

The impact of contemporary industrial civilization upon the village world is the problem that I propose to explore, a problem of primary importance to us. To demonstrate this, I should like to describe an anthropologist's field trip to the exotically remote Ecuadorian village of Nayon. Here, scarcely six miles from the Equator, one can see in microcosm some of the problems that today face the industrial world and the yet uncounted millions of village dwellers.

The Village of Nayon

The romantic may find it disconcerting to arrive by airplane in Quito, the capital of Ecuador, a rapidly growing city approaching a quarter of a million. But soon the suburbs are passed and one comes unexpectedly to the top of a steep escarpment overlooking one of the great central valleys of the Andes. A thousand feet below lies the village where slightly over 2,000 souls live among their farm lands in the apparent changeless peace of centuries. To the west, Nayon is isolated by the heights from which it is viewed, and in other directions it is separated from its neighbors by deep, rugged barrancas traversed only by rocky trails.

At first glance little has changed or will change here. People are at work in the fields with oxen and wooden beam plow, or simple spade and hoe and machete. An elevation of 8,500 feet provides a mild climate with occasional frosts, despite the nearness of the Equator. Miles across the valley the great eastern range rises in vast bleak *paramo* where sudden blizzards may trap the unwary

traveler. North, east, and south, glacier-crowned volcanos overlook deep barrancas where tropical vegetation flourishes. Warmed by tempered tropical sunshine, its rich volcanic soil watered by 60 inches of rain, its lands relatively free of the dominance of the hacienda, the romantic might expect Nayon to be an idyllic representative of the self-contained independent agricultural village. Nothing could be further from reality, for the industrial-urban world has pressed hard upon it for over a quarter of a century. Let us see what this means.

The people of Nayon are Quechua-speaking Indians, as are their neighbors. Under missionary control after the Spanish conquest, Nayon was organized as a parish. The present townsite is in the typical colonial Spanish grid pattern of rectangular blocks about a central plaza, on which face the church and government offices. About half the population of the parish lives in the townsite. The grid of streets and trails at present includes approximately half of the most fertile land in the parish.

Until about thirty years ago, the village seems to have been a self-sufficient agricultural community with a mixture of native and sixteenth century Spanish customs. Lands were abandoned when too badly eroded. The balance between population and resources provided a minimum subsistence. A few traders exchanged goods between Quito and the villages in the tropical barrancas, all within a radius of ten miles. Houses were dirt-floored, with thatched roofs, and pole walls, sometimes mud plastered. Guinea pigs ran freely about each house and were the main meat source. Most of the population spoke no Spanish. Men wore long hair and ponchos and concerned themselves chiefly with farming. Most formal controls and external relationships were managed by a resident parish priest, but informal leadership and familial controls governed social relationships. From the consolidation of the Spanish conquest until about 1920, no significant change occurred. In short, people lived within an integrated, internally consistent system of social relationships, habits, customs, values, and living techniques which were satisfactorily in balance with ecological conditions, and which supplied the necessary requirements for survival of the society.

Recent Changes in Nayon

The completion of the Guayaquil–Quito railway in 1908 brought the first real contacts with industrial civilization to the high inter-Andean valley. From this event gradually flowed not only technological changes, but new ideas and social institutions. Feudal social relationships no longer seemed right and immutable; medicine and public health improved; elementary education became more common; urban Quito began to expand; and finally—and perhaps least important so far—

Typical vegetation in the barrancas.

Arrival of bus from Quito. Church in background.

One of two remaining old-style houses.

modern industries began to appear, although even now upon a most modest scale.

In 1948–1949, the date of our visit, only two men wore long hair, and only two old-style houses remain. If guinea pigs are kept, they are penned; their flesh is now a luxury food, and beef is the most common meat. Houses are of adobe or fired brick, usually with tile roofs and often containing five or six rooms, some of which have plank or brick floors. Most of the population speaks Spanish. There is no resident priest, but an appointed government official who, with a policeman, represents authority. A six-teacher school provides education. Clothing is becoming city-like; for men it often includes overalls for work, and a tailored suit, white shirt, necktie, and felt hat for trips to Quito. Attendance at church is low and many festivals have been abandoned. Volley ball or soccer is played weekly in the plaza by young men who sometimes wear shorts, blazers, and berets. There are few shops, for most purchases are made in Quito. Much of the farm produce is sold in Quito, and from there comes most of the food, so that there is a far more varied diet than twenty-five years ago. There are piped water and sporadic health services; in addition, most families patronize Quito doctors in emergencies. Since 1949 the road has been paved, and bus service is more regular. There is one reputed millionaire (in sucres, the national currency, and the equivalent of about $150,000 U.S.), and several are classed as wealthy.

Thus, although to the casual observer Nayon still seems a timeless, sleepy farm village, in little more than a quarter century the changes in the direction of what North Americans call progress have been enormous. Let us examine the meaning of these changes in terms of the pattern of living.

Loss of Self-Sufficiency

The changes in housing and clothing mean greater dependence upon the outside world. Masons must be hired to lay adobe or brick or stone walls, and so a new kind of specialist has come into being, one who works for wages. For fine work, outside craftsmen are imported along with such materials as cement and tile. Sewing machines are now considered necessary, although much clothing is purchased.

Except for some new specializations, tools for the primary occupation of farming have undergone little change. The wooden beam plow drawn by oxen, and the dibble, hoe, shovel, mattock, and machete are the universal tools. But the crops and their use have undergone notable change. Maiz or Indian corn is still the primary crop, but very little is harvested as grain. Almost all is sold in Quito as green corn to eat boiled on the cob, and a considerable amount of the corn eaten as grain in Nayon is imported. Beans, which do poorly here, are grown on a small scale for household consumption. Though some squash is eaten, most is exported. Sweet potatoes, tomatoes, cabbage, onions, capsicum peppers, and, at lower elevations, sweet yucca or arrowroot are grown extensively for export; indeed, so export-minded is the community that it is almost impossible to buy locally grown produce in the village. People cannot be bothered with retail sales. Although areas devoted to fruit are small, quantities in excess of household needs are sold in Quito. Oxen are kept for plowing, but there is no dairying; milk, if used, is brought from Quito. Donkeys, mules, and horses are kept by some as pack animals. A few people buy shoats and fatten them, but not many pigs are butchered locally; again they are sold in Quito. A few others do the same with cattle, buying in Otavalo to the north, fattening, and then selling in Quito.

Clearly, then, Nayon is no longer a self-sufficient village. It is now deeply enmeshed in the money economy of a larger region, and especially with the city of Quito. The extent of these involvements is perhaps best underscored by the foods imported in significant amounts, as revealed by a thirty-day check of six families. (Table 1). In a thirty-day period these six families purchased outside the village most of their animal proteins, all fats, and a substantial part of their carbohydrates. Moreover, should outside sources be cut off, only the starches

TABLE 1

FOODS IMPORTED INTO NAYON IN SIGNIFICANT QUANTITIES

Beef	Milk	Peanuts
Cheese	Bean Flour	Lard
Bread	Spaghetti	Brown sugar
Oatmeal	Some corn	Some fruits
Barley Flour	Potatoes	Chocolate
Rice		

from corn, manioc, sweet potatoes, and squash could be supplied locally. The village shows the dependency, insecurities, and frustrations attendant upon a national and world economic system. Expressed in money values, these six families purchased from thirty-seven per cent to seventy-four per cent of their food. In absolute figures, the least spent in purchasing food supplies was $26.00, the most $60.00. In addition, during the month many meals were purchased away from home.

The sale of most local farm products and the purchase of imported foods, clothing, and building materials gives new importance to transportation. Although many goods still travel by pack animal or by human carrier, increasing amounts are transported by bus and truck so that a major community undertaking was the construction of the road across the rugged and steep hills to Quito.

Changes in Family Life

In the village world the family is characteristically one of the most important social units. Usually the family is extended to include a larger group of relatives than in our society, and close ties exist with related kin groups. People live primarily in contact with relatives. The individual is reared within the family, and it provides his security system and his major social life. Economically, the family tends to be the basic unit of production and consumption.

With these points in mind, the activities of six families were recorded in detail for the period from June 15 to July 15, 1949. In one family, consisting of the parents, an adolescent boy and girl and four children in primary school, the father spent 13 days in Quito during this period and another 13 days away on a trading trip to the Amazonian lowlands. The mother spent 12 days in Quito and four other days away from home on trips. The father was absent for 71 meals out of a possible 90, the mother was absent for 27 meals. The mother spent six days on farming activities during the month, the father none. The goods carried by the father on his trading trip had a wholesale value of over $1,200. Among household expenditures we find nearly $100 spent at one time to provide new outfits for the children in school to wear at the closing exercises at the end of the school year.

In a less active family, specializing in buying and shelling peanuts for the Quito market, the father was absent on business 15 days out of the month, the mother six days. Incompletely recorded figures on gross sales amount to about $2,000 for the month. In a family with older children, specializing in buying and selling fruit, the father was absent 15 days, the mother eight days, and the two older sons and the oldest daughter were absent 19, 14, and 15 days respectively. Other family records are comparable.

These examples suggest how thoroughly many Nayon families are involved in the market economy. Agriculture played but a small part in their activities, although all would describe themselves as farmers. Clearly the social relationships of Nayon are no longer confined to the village. Indeed, impersonality characterizes many of the social contacts within the village. In the detailed schedules, we find only a few visits to relatives and only one visit to a non-relative. Characteristically, people in Nayon do not know what their neighbors are doing; moreover, they have comparatively little interest in knowing. Members of the community are judged on their public conduct, especially in relation to public affairs. Gossip and discussion there is, and opinion may be mobilized for or against individuals, and sharp conflicts exist. But, essentially, social relations at Nayon are impersonal in a form more characteristic of the city than of the village.

A modern house.

Visitors to Nayon showing dress once common in Nayon.

Changing Social Relationships

These attitudes are well illustrated in relations with government, represented primarily by a political agent appointed by the federal authorities. When new laws permitted Indian communities to recommend their own choice of political agent, Nayon immediately closed ranks when its recommendation was ignored. The new political agent contributed to this by unusually arbitrary actions. Consequently, not only did residents cease to make the customary gifts, but all refused to sell him food or services. The owner of the bus line refused him passage. As a result, the political agent had to walk to Quito and back, not only on official business but to purchase all his family's food supplies. As an ultimate outrage to a Latin American male, when the water supply failed he had to carry water from the barranca—an indignity even the foreign ethnographer was spared. But in addition to these essentially village-like procedures, community members kept lists of the fines levied, searched government records to prove that the political agent had not paid the proper amounts into the municipal treasury, and brought charges of embezzlement against him which resulted in his removal from office and replacement by the village nominee. In the same year the village also brought about an inspection trip by the mayor of Quito, pavement of the road, the contribution of materials and engineering supervision for extension of the water system, and the public promise of electric lights as soon as additional transformers became available.

This looks like, and is, unity in action of a very efficient sort. Without going into the complex details involved, the significant point is that the accomplishments were political in nature and were achieved by effective political organization of urban type. Men who saw little of each other under other circumstances—that is, men who were not friends or who might in some respects actively dislike one another—utilized sophisticated political machinery effectively to achieve goals which in considerable part involved the improvement of urban-type facilities. Essentially, their action was that of a neighborhood improvement association in a large city, in marked contrast to the helpless bewilderment and internal bickering found when villages unfamiliar with urban ways are faced with new external problems.

A characteristic of the village is its homogeneity; there are few specialists and most people share the same skills and knowledge. Nayon, once primarily agricultural, is today a village of traders. The examples given are not extreme, for some traders handle single transactions of $6,000 or more. Others travel from Peru to Colombia. In addition, we find scattered individuals who are oil-well riggers, construction workers of at least medium skills, and mechanics. There are also two school teachers. No Nayon youth has yet reached the University, but half a dozen are in advanced technical schools and one is in the Ecuadorian naval academy. And, to keep the picture in some sort of balance, mention should be made of the persistence of older kinds of specialists such as blanket and belt weavers, and the development of a small, essentially landless class

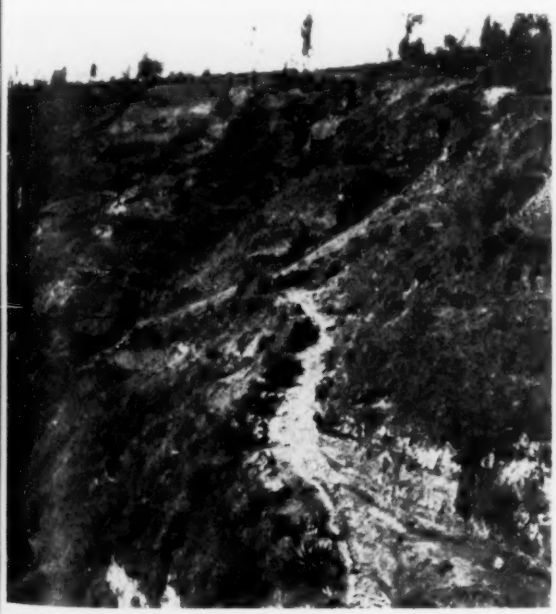
A uniformed policeman represents modern authority

which survives as unskilled laborers, mainly on construction jobs in Quito.

The nature of Nayon religious life and participation has likewise changed. Nayon is still Catholic in the main, and the few who have become Protestants are disliked and distrusted. Indeed, despite the intelligent and wholehearted assistance of the parish priest, the greatest obstacle to securing cooperation for the Nayon study was the persistent suspicion that we were Protestant missionaries. Nevertheless, religious influence is declining in Nayon, especially among the younger people. The parish priest today must care for half a dozen parishes and no longer resides in Nayon. Few, indeed, wish him to. Although mass is held every Sunday, attendance is usually small. Young men rarely participate, although they do not start their Sunday games of volleyball, soccer, or pitching coins until mass is concluded. On occasion though, they have spoken rudely to the priest. Most religious festivals have been abandoned or reduced to celebration of the mass. Those still retained become increasingly secular, with emphasis upon social aspects, drinking, and dancing. Even the festival of the patron saint is a poor thing, shorn of most of its color and traditional folk dances. Religion in Nayon is essentially as formal and as restricted in its function and meaning as in any city.

Penalties of Success

So far, perhaps, this account sounds like a success story entitled, "Indian Village Adapts Rapidly to Modern Life." Many members of the group have accepted major shifts in the socio-cultural system with little difficulty and look forward to additional changes. Most people in Nayon are conscious of change and of its attendant difficulties, and believe they have met the situation well.



A trail across the north barranca.

View eastward across the village toward the volcano of Pichincha. Eroded and abandoned land in foreground.

But a major finding of modern anthropology is that culturally established technologies, behavior patterns, value systems, and social structures tend to form closely integrated socio-cultural systems. If this be true, have Nayon people understood all the ramifications of change? Has all the population adapted equally well? Let us look for some entries on the debit side. What are the problems Nayon faces, and what are the forces released by its abandonment of a way of life that has been moderately successful for four hundred years? And what has impelled Nayon citizens to make this change? These are questions Nayon shares with the thousands of villages on every continent.

One characteristic of the village world is the existence of a balance between resources and population. Birth rates and death rates are in balance unless nearby cities absorb surplus population. Living standards tend to be relatively unchanging. In Nayon improved health conditions, erosion, and new living standards have between them destroyed this resources-population balance.

On the surface, it seems difficult to maintain that health conditions in Nayon have improved. Inadequate surveys of school children indicate that at least sixty to eighty per cent of the population is infested with one or more intestinal parasites.

Goiter, avitaminoses, and other forms of malnutrition are prevalent. Measles, mumps, and chicken pox are considered normal for all children. Tuberculosis, malaria, and venereal disease are increasing. On the other hand, smallpox and whooping cough are fairly well controlled.

Unsatisfactory as the health situation may appear by our standards, the best test of it is the relationship between births and deaths. From the beginning of the registration of vital statistics in Nayon in 1936 through 1948, there were 287 more births than deaths. This is a population increase of nearly thirty per cent in thirteen years.

Man and the Land

Natural resources have diminished as population has increased. Great gullies are destroying the most fertile land. Sheet erosion has removed all the top soil from large areas, exposing a hard and infertile clay subsoil. In areas under cultivation each torrential tropical rain removes top soil with frightening rapidity; streets are becoming gullies. Consequently, much land has been abandoned, and even the forested areas have been planted recently with the alien eucalyptus.

Diminishing resources and expanding population have caused excessive fragmentation of land holdings. This process is clearly shown in Table 2. In the oldest generation couples owned on the average 9,192 square meters (about two acres) in the central area. They have sixty-four living grandchildren who will inherit on the average 1,757 square meters. Only five of the wealthier grandchildren are married but they already have 32 living children. As additional children will probably be born to these parents, even the most fortunate group of children of the fourth generation probably will inherit less than one-eighth of an acre on the average.

View across a barranca showing islanded remnants of cultivable land.

TABLE 2

LAND HOLDINGS IN NAYON
Central District—12% of Population

Generation	I	II	III	IV
No. Individuals	7	24	64 (some families incomplete)	32
No. married	7	24	7 (5 with children)	0
Average inheritance of married couples (sq. m.)	8,054	4,415	7,189	—
Total land owned, including purchases (sq. m.)	9,192	5,264	Incomplete	Incomplete
Average potential inheritance, not including spouses' land (sq. m.)	—	2,681	1,757	1,485*
Range of actual or potential inheritance (sq. m.)	2,205 to 12,789	—	—	0 to 2,668

* For present offspring of 5 families with children. All families incomplete. All families above average in inheritance.

Another measure of pressures on land is the changing price structure. Although the figures in Table 3 are uncorrected for inflation, they are striking. The greater increase for land near the plaza reflects the adoption of urban ideas; except for the largest cities, residence on the central plaza carries the highest social prestige in Latin America.

These few data make it clear that the traditional balance with the environment has been destroyed at Nayon. Even if improved agricultural techniques and erosion control are introduced, approximately one-half of the Nayon children who reach adulthood must emigrate if the village is to maintain its present relatively low living standards on the basis of agriculture. Moreover, the Nayon standard of living is rising, and aspirations for a continued betterment are strong. Any further improvement of health conditions must mean an even greater emigration rate. Alternatively, other sources of livelihood must be found.

To some degree both of these solutions are already in operation. A few families have emigrated, mostly to the coastal lowland areas now being developed in Ecuador. Others have purchased land elsewhere and may soon migrate. This trend probably will be accelerated. Many have resorted to

TABLE 3

CHANGE IN LAND PRICES IN NAYON

	No. parcels	Average price per square meter (dollars)	Percentage increase above pre-1918 average
Sales before 1918	7	$0.0052	
Sales, 1943–1948, total	7	$0.1521	2,925%
Parcel nearest Plaza		$0.6090	11,711%

more extensive trading and entrepreneurial activities. Others work for wages in the nearby city.

The economic problems created in Nayon by increasing population, diminishing land base, and demands for a rising living standard are worldwide in scope. Whether on Pacific islands, in Africa, or in Asia—wherever modern public health has begun to penetrate, or where outside agencies have put a stop to such population-limiting devices as war, famine, and infanticide—population is pressing hard upon resources and native technology. Everywhere, too, the solutions being tried are creating new social problems, for the fabric of any society and its culture is composed of functionally interrelated parts; alteration of any significant part must affect all the others.

New Social Problems

In Nayon some of the social problems have already been recognized by Nayon residents. Wage labor in the city is clearly seen as leading to personal disintegration, alcoholism, and low living standards. Most parents will make great sacrifices to keep their sons from being forced into wage-work. Perception of other problems is still rare.

Emigration destroys community ties and further diminishes the importance of the extended family. Moreover, those who emigrate move into new kinds of communities where traditional guides to behavior no longer operate and where greater impersonality characterizes social relationships. The individual is more on his own.

The social effects of trading enterprises are even less clearly seen by the Nayon population. Not only do women take some part in trade, with long absences from home, but they also are carrying on an increasing amount of the agricultural work. Today it is not uncommon to see women plowing—a sight very rare, if not unknown, ten or fifteen years ago. Small children are commonly relegated to the care of older siblings, who at early ages may be in complete charge of the home for large parts of the day. More and more the school is a major factor in the training of the young, and it is inculcating the values of the city rather than of the village. Even in child training, the church now occupies only a minor role in the community.

The long absences of parents and the increasing shift to a money economy result in a great decrease of mutual aid and cooperative activities. The *minga* or cooperative labor for public improvements seems clearly destined to disappear before long. There is still some cooperative work among friends, neighbors, and relatives, but there is a marked decrease in aid that now can be measured in money. Individualism and self-reliance are prized; many will not visit the free health clinic because it is "charity."

Future of the Village

Because of its nearness to Quito, Nayon will soon be engulfed in the expanding and industrializing city. Most of its population either will emigrate to other agricultural zones, enter upon commerce and other urban occupations, or become merged in the urban proletariat. It is conceivable, indeed, that in the not too distant future the leaders in commerce and industry may come from such Indian communities as Nayon. In Ecuador, as in much of the village world, the dominant elite both shun labor as degrading and give low social status to the new occupations of trade and industry. Hardworking, shrewd, and enterprising, the traders of Nayon seem indeed more likely to develop into the leaders of industry in Ecuador than do the members of the entrenched elite. Past history, however, does not suggest that the latter will surrender their positions of power without a struggle.

A typical cornfield in center of village.

The implication of the picture of Nayon that I have presented is that the village will adapt to the industrial-urban society emerging in Ecuador. But problems of individual adjustment and the continued functioning of the village socio-cultural system will become increasingly difficult. The old trading background has made it relatively easy to adjust to trading over a wider area and on a larger scale. If individuals own land they still can pretend they are farmers. However, they are only beginning to recognize the resultant effects on children whose fathers are away for long periods and whose mothers must frequently leave them alone for the better part of many days. Nor, as the population outstrips its land base, are young people deceived. Already some ask: "Without land to be farmers or capital to become traders, what are we to do?" Too educated to accept the role of unskilled laborer in Quito, too little educated for better jobs, still barred as Indians from skilled jobs by the persisting caste system, they go increasingly to distant places, to the coast or to the oil fields, where they may escape identification as Indians.

In my discussion of Nayon I have tended to emphasize technological and material changes and some of their concomitant social changes and problems. It is true that these are more easily observed and presented, but perhaps far more important are shifts in values, attitudes, and ideologies. Herskovits has pointed out that "no economy has been discovered wherein enough goods are produced in enough variety to satisfy the wants of all the members of any society."[1] He might have added that no society, including our own, has exhausted the capacity of all its members to develop new wants. Industrialism has, among other things, developed the most effective machinery yet known not only for the creation of new wants, but also for the transmission of new ideas, goals, and values. Consequently, ideological problems of transcendent importance now exist on a worldwide scale.

Throughout the village world, men are finding their old value systems unworkable or are groping toward new stystems and new social relationships. As Opler and Singh remark of changes occurring in a village in Northern India:

> When we come to the question of the areas in which the greatest amount of change has taken place, it is our impression that the most extensive and pervasive shifts have occurred in outlook and political and social relations rather than in the realm of technology, possessions, and work habits. The few metal plows, mechanical chaff cutters, improved sickles, and bicycles which have appeared in the village, while important, are, perhaps, more significant as tokens of things to come than as immediate practical assets. But the mass education of "untouchable" children, the introduction of co-education, the tolerance of widow remarriage, the wresting of political power from the land owners and high caste groups by the artisans and lower castes —these are momentous changes that penetrate to the core of village life, and indeed, of Indian life. They bespeak the end of an era and a reorganization of intellectual and social energy.[2]

Group assembled for mass and procession during festival of patron saint.

Nature of Cultural Revolutions

Events in Nayon, then, have significance primarily as an example of the effects of a worldwide cultural revolution in which industrialism is altering the technology of the village. Through its communication systems, industrialism is carrying urban patterns of life and thought into every part of the world, with far-reaching effects upon village social life. The basic processes of this cultural revolution are not new to human history. The village world itself was a product of the Neolithic revolution, some ten thousand years ago, that converted man from a hunter and a gatherer into a village-dwelling food-producer. Like the Neolithic revolution, the urban-industrial complex had its center of origin, its period of growth toward an integrated system, and its subsequent rapid and inexorable spread over a large part of the earth's surface that involved most of the world's population. The events through which we are living, differ primarily only in magnitude and speed from processes which have occurred many times in human history, from the discovery of fire to the discovery of metallurgy.

Festival of the patron saint. Notice the small crowd and lack of pageantry.

The urban-industrial revolution nevertheless carries with it one new ingredient, the methods of science in the solution of problems. It is not my contention that the social sciences can stop the present cultural revolution any more than some primitive philosopher could have prevented the Neolithic revolution. Nevertheless, the social scientist today has techniques for understanding the processes involved. Once the processes are understood, both in their general form and in their particular application, social science today can define various alternatives which may lessen the impact of the revolution upon the village world and permit rational choices between various adjustment patterns.

Basically, the problem confronting our times is whether the urban-industrial revolution shall take place under democratic or authoritarian auspices. In the past the village world has fitted into both types of pattern. The internal patterns of relatively independent villages tend on the whole to be democratic. Where the village has fitted into a pre-industrial society dominated by urban centers, the authoritarian solution has frequently occurred. Often such societies are accompanied by relatively rigid class or caste groupings of population. India is an extreme example, but class groupings were markedly developed in feudal Europe and still persist in many parts of Latin America and Asia. Consciously or unconsciously, it is such patterns that Nayon is endeavoring to break, with some success.

If Nayon represents a relatively successful adaptation to the contemporary cultural revolution, anthropological literature is full of less successful examples. In some cases the difficulties of adjustment have led to rejection and attempts to return to the past. Such a nativistic movement characterized some aspects of Ghandi's program in India. Alternatively, the frustrations attendant upon a changing culture have frequently resulted in a blind nationalism which places all blame for difficulties upon the already industrialized West.

The industrial-urban revolution, as we have seen, brings with it far-reaching changes in social relationships and institutions. At best it calls for more varied types of adjustment by the individual, and in the village world in transition it often creates intolerable frustrations. Under such circumstances the individual may reject reality and retreat to the self-created world of the psychotic. Another alternative is to surrender to authoritarian leadership, a solution usually accompanied by deep emotional appeals and increasing recourse to scapegoats to relieve frustrations. The democratic solution is to develop individuals with that confidence in themselves which allows them to respect others, coupled with individual responsibility and great capacity to adjust to new situations.

Whether the village world will turn to the authoritarian or the democratic solution of the problems created by the present cultural revolution depends in large measure upon how rapidly the newly created aspirations in the village world can be satisfied and its adjustments facilitated. Men of the village world will turn in those directions which seem to offer them the greatest satisfactions. The social sciences may yet be immature, but it is my contention that today they have the tools to discover the major problems, to make apparent the predictable directions of change, and to present clearly the available alternatives. Upon our ability to understand the nature of the cultural processes at work today, and upon the efficiency with which we meet the problems they raise for Nayon and its thousands of counterparts, may depend our future for centuries to come.

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Where do Cosmic Rays Come From?

ARTHUR BEISER

The author is completing work for his Ph.D. degree at New York University, where he is a director, Nuclear Emulsion Research Project; Field Director, Cosmic Ray Research Project; and part-time instructor in Physics. He has just returned from helping establish a cosmic-ray laboratory at Mount Wrangell.

WHEN A. GOCKEL took off in 1910 from Switzerland in a balloon loaded with instruments he expected to find that the radiation they recorded decreased as he got farther and farther away from the earth and its radioactive mineral content. This seemed a reasonable prediction since radiation is absorbed in the atmosphere. To everyone's surprise he found that the radiation intensity *increased* with altitude; in the next few years this result was confirmed by other investigators. But if the thorium, uranium, and radium in the earth's crust did not produce the observed effect, what did? Victor Hess suggested in 1912 that radiation of some kind was reaching the earth from somewhere (or everywhere) outside, and in the twenties these still mysterious rays were termed "cosmic" rays by Millikan. Today it is known conclusively that cosmic rays indeed exist, it is known what they are when they enter the earth's atmosphere from space, and it is known how they interact with the atmosphere to produce various kinds of secondary radiations. But, after forty years, we know only what Hess knew about the origin of cosmic rays—they come from "outside."

To begin with, primary cosmic radiation, that is, the rays as they exist in space, is composed of atomic nuclei traveling with speeds so enormous as to approach that of light (186,000 miles per second). The relative proportions of the different atoms parallel those found in nature generally: thus the majority are hydrogen and helium atoms, with heavier elements such as carbon, nitrogen, and iron also present but in much smaller numbers.

Before these particles can reach the earth's surface they must pass through the atmosphere. The blanket of air covering our planet is heavier than many realize—equivalent to a layer of water thirty-four feet thick. Even the tremendous energy of the primary cosmic rays is not sufficient to enable them to get through this much matter unchanged. However, the debris resulting from their collisions with air atoms does reach the surface of the earth and in fact has even been detected several hundred feet underground. This debris, in addition to the protons and neutrons of which the struck atoms are composed, includes mesons, unstable particles associated with nuclear structure that are not very well understood at present, gamma rays, like those given off by radium only more penetrating, and positive and negative electrons.

The number of cosmic rays actually reaching the earth is very small, and the rays are detectable only with highly sensitive instruments. Hence they are entirely harmless on earth, although there is some question what their effect would be on people traveling through space for long periods of time.

The cosmic ray stream in its progress through the atmosphere presents a marvelous and exciting picture of one aspect of the workings of the physical universe. But this particular picture is not mysterious any longer, although, to be sure, there are many details that are not entirely clear at present. It is the element of mystery still surrounding the origin of cosmic rays, not only where they come from but also how they acquire their fantastic energies, that gives to research in this field a unique sense of adventure quite unfamiliar in other regions of scientific endeavor. Therefore we will not go any farther into the behavior of cosmic rays near the earth, but will concern ourselves exclusively with their "cosmic" aspects about which so little is known.

We have already called the energies associated with cosmic radiation "tremendous" and "fantastic." To justify these words one need only look at the highest particle energies attainable in the laboratory, by which is not meant the traditional small basement room of the cloistered scientist but rather the factory-size structures becoming more and more common in modern physics research. The unit of energy in nuclear physics is the electron

volt, abbreviated ev. The new four million dollar cosmotron accelerator at Brookhaven National Laboratory on Long Island, the largest now operating, contains 2200 tons of iron and 70 tons of copper, is over sixty feet in diameter and uses forty million watts of electric power. It is ultimately expected to produce protons of three billion ev (or three Bev), and an even larger machine being constructed at the University of California is planned for about six Bev maximum energy.

But energies as high as 100,000,000 Bev have been found in cosmic rays!

The measurement of such high energies is an interesting subject in itself. Less energetic particles can be examined by using "cloud chambers" with magnetic fields applied. The paths of particles passing through such a device can be photographed as they are bent by the action of the field, and their energies can be determined with fairly good accuracy from their curvatures. A more indirect method is required for a particle in the Bev range, however, based on the avalanche of secondaries it creates in the atmosphere. These secondaries, mostly mesons, tend to spread out as they go through the air, much like an avalanche of snow. The daughter particles of one primary ray may cover an area of several acres of the earth's surface. Cosmic ray shower theory tells us how to relate the width of such a spray of particles to the energy of the primary that initiated it. By observing several Geiger counters widely spaced—when they all record the passage of particles through them at the same instant, a giant avalanche must have been responsible—the primary energy can be calculated from the known counter spacings.

No information is available at present on the existence of energies greater that 100,000,000 Bev, because the enormous counter spacings needed to detect them introduce complications in the associated electronic instruments that so far have not been satisfactorily solved. However, a continuous spread of energies is present between the lowest that are detectable and the highest, and so there is good reason to suppose that even more energetic cosmic rays exist than have been measured thus far.

Now "outside," where cosmic rays come from, is a pretty big place. If we can narrow it down to a specific region of the universe, for instance the solar system or our galaxy, we will have simplified the problem of their origin greatly and perhaps even have gotten some hints about the mechanism that generates cosmic radiation.

Consider first the solar system and its principal body, the sun. Nearly all the energy reaching the earth as light and heat is provided by the sun, and so it is natural to think of it as the source of cosmic rays as well. But if this were the case, cosmic ray intensity would vary from a maximum during the day to almost nothing at night, just as with the light rays of the sun. No such marked variation is observed. Add to this the inability of physicists to conceive of a convincing solar mechanism for producing them and the result is clear: cosmic rays do not come from the sun.

Other things besides light and heat do come from the sun. Low-energy electrons and protons (hydrogen nuclei) are emitted by it at times in vast streams that cut across the earth's orbit. Their interactions in the upper atmosphere produce the startling and beautiful phenomenon of the aurora, the "northern lights."

It should be mentioned in all fairness that there are a number of physicists who still are sure that a solar cosmic ray origin is possible.[1] They contend that magnetic fields could exist in the solar system that would "scramble" the cosmic rays so that as many would reach the night side of the earth away from the sun as reach the day side toward the sun. Now magnetic fields that could do this are certainly conceivable; however, such fields, if they could scramble particles with energies of millions of Bevs, must be so strong as to permit their detection on the earth. And no magnetic fields in the solar system stronger than the relatively feeble one on the sun itself have ever been found.

The next subdivision of space to be looked into is our galaxy. Our galaxy, the Milky Way, is shaped like a fried egg, with a dense concentration of stars in the center that becomes more and more spread out toward the edges. The entire galaxy, roughly 100,000 light years across, rotates. Since the solar system is two-thirds of the way out from the center, this rotation means that we are traveling through space with a speed of 180 miles per second (superimposed, of course, on our motion around the sun). The apparent direction of this flight through the sky is the constellation Cygnus, the Swan.

In 1935 Arthur H. Compton, now Chancellor of Washington University in St. Louis, and Ivan Getting proposed an experiment based upon the rotation of our galaxy to determine whether or not cosmic rays originate somewhere inside it.[2] If cosmic rays come from *outside* the galaxy, either from other galaxies, or spiral nebulae as they are sometimes called, or from extragalactic space, then more of them will strike the forward side of the earth than the backward one. Just as in the case of solar particles this means that cosmic ray intensity should

vary every twenty-four hours, only to a much smaller extent since many rays still could enter at night. The magnitude of this variation, somewhat less than one per cent, and the time of maximum, 20 hours 40 minutes sidereal time, were predicted by Compton and Getting, whose calculations were subsequently modified slightly to take into account the disturbing effect of the earth's magnetism on the paths of incoming cosmic ray particles.[3]

Slight variations in cosmic ray intensity definitely exist. Unfortunately for the extragalactic origin hypothesis these variations result from changes in the temperature and pressure of the atmosphere, and can be predicted from a knowledge of these quantities as measured with standard Weather Bureau meteorological balloons. No correlations with the Compton-Getting calculations have been found; most, if not all, cosmic rays therefore originate within our galaxy.

At this point, although we still do not know anything substantial, the problem of cosmic ray origin is at least out in the open where we may hope to attack it realistically. We know that a galactic mechanism of some sort is probably responsible, and that it must be capable of generating with extremely high energies atomic nuclei as heavy as iron. Precisely how does this generation process work?

Serge Korff computed twenty years ago that the "energy density" of cosmic rays in space was approximately that of starlight, about 10^{-12} ergs/cm^3, light also being a form of energy.[4] This conclusion is still valid, and it poses a further question that must be added to the above one: is it possible to conceive of a mechanism that can give to cosmic radiation as much or more total energy than the entire light output of all the stars in the universe? To be aware that the magnificence of the heavens on a starry night is caused by rays of light that took thousands and even millions of years to reach the earth is to be aware of the colossal scale of this energy output. Now we are asked to believe that invisible, all but undetectable particles swarm through the Milky Way and are just as important in the scheme of the cosmos as the familiar and ever present stars. We can do nothing but believe this, of course, and it might be pointed out to the romantics among us that any number of scientists have found the contemplation of cosmic rays fully as satisfying an emotional experience as poets have found in gazing at the visual wonders of the sky.

Two possibilities exist, at first glance, for cosmic ray generators. Robert Andrews Millikan, who received a Nobel prize for the first accurate measurement of the charge on the electron, one of the fundamental constants of nature, suggested that cosmic ray particles get their energies all at once, in single acts. He proposed that this act was the annihilation of atoms and the complete conversion of their mass into energy according to Einstein's formula that energy equals mass times the square of the speed of light. This energy, in the form of a gamma ray, would presumably strike another atom in space and be transferred to it, giving the cosmic rays that we are familiar with. For example, the so-called mass energy of a helium atom is 4 Bev; of a carbon atom, 12 Bev; of an aluminum atom, 27 Bev. The heaviest atom known outside the laboratory is that of uranium, with a mass energy of 120 Bev.

This is a lot of energy. However, it is not nearly enough to account for cosmic rays, and Millikan's theory, after a period of doubt, had to be discarded.

Another single-act idea is startling in its simplicity. The universe is expanding, and it is thought that this expansion results from the explosion that was the creation of the universe about three billion years ago. Could not cosmic radiation be radiation left over from that primal event? It is hard to say in the absence of detailed information about the formation of the universe whether this suggestion should be taken seriously, but no great enthusiasm has yet been manifested in its favor. The principal difficulty lies in the history of such rays in the three billion years since they were assumed to be created; there is a good chance that all of them would long since have been absorbed by interstellar matter.

The second possibility is that the particles acquire energy gradually, a little at a time, until their ultimate energies reach the enormous values that seem to be characteristic of this radiation. Specific processes that could do just this are known that need no unusual assumptions to justify their existence and operation. It is a matter of examining them in detail and choosing the one or ones that seem adequate; something, it should be stated, that is easier said than done.

Literally dozens of schemes have been proposed for accelerating cosmic rays by electric and magnetic fields somewhere in space. Electric fields, which would act to speed up charged particles in much the same way as the high voltages used with television picture tubes speed up the electrons whose impact on a luminescent screen produces a visible spot, can be ruled out at once. Space is a good conductor of electric charges since it is empty, and so positive and negative charges will

come together and neutralize each other very rapidly if they exist separately at any time. An electric field is present around an isolated charge; if no such charges exist neither can the field, and fields that do not exist can have little to do with cosmic rays.

The situation is more promising when we come to magnetic fields, in particular varying magnetic fields. These fields can occur in individual stars or in the diffuse clouds of interstellar matter that float through space. Nothing that we know contradicts the possibility of particles with cosmic ray energies being generated in stars, for instance in giant "spots" like the sunspots of our sun, or in Babcock stars which have large and rapidly varying magnetic fields. But the number of stars that can generate cosmic rays by these mechanisms is nowhere nearly enough to account for the total number of cosmic rays present in space.

Little is really known about magnetic clouds in the galaxy; perhaps this is why theories of cosmic ray origin based upon their supposed properties seem to be very promising. Now clouds of matter are almost certainly present in space, and it is not unlikely that they have magnetic fields associated with them.[5] Fields of only about 10^{-6} gauss are sufficient for the acceleration process described below. A cubic inch of such a cloud may contain several atoms, whereas a cubic inch of air consists of atoms numbering ten followed by more than twenty zeros! Compared with ordinary space, though, such cloud densities are appreciable.

These clouds move with velocities of around twenty miles per second. A moving charged particle that collides with an irregularity in a magnetic cloud caused by turbulence either increases or decreases in speed, an excellent analogy being a tennis ball striking a moving racquet. If the racquet is moving away from the ball when it hits, the ball will rebound much more slowly than it was going before it touched the racquet. If the racquet is moving toward the oncoming ball, the ball goes faster meeting a magnetic cloud experiences a change in its speed and hence in its energy. By the application of a fundamental law of physics known as the principle of equipartition of energy it can be proved that, on the whole, more particles gain rather than lose energy in encounters with such clouds. After enough collisions, enormous energies can be produced.

The above ideas were thought about more or less vaguely for many years and were finally put into concrete mathematical form by the famous nuclear physicist and Nobel prize winner Enrico Fermi.[6] It is definitely possible that this process contributes most or even all of the protons present in cosmic radiation. And protons make up about ninety per cent of the cosmic rays reaching the earth. The trouble is that Fermi's theory fails completely to explain how particles heavier than hydrogen atoms can be accelerated to high energies.

The reason for this state of affairs lies in the fact that heavy particles lose energy more rapidly in their travels than light ones do; in order not to be stopped completely before receiving another "kick" from a passing cloud an iron nucleus, for instance, must have an initial energy of three hundred Bev. For protons the comparable figure is only two hundred Mev (million ev, not billion ev), and such comparatively low energies, in the cosmic sense, may be produced in various ways. But three hundred Bev is another story, and no one has yet been able to think up a way of arriving at energies like this *before* magnetic clouds can take over the work of acceleration.

This is where the subject stands today. Very little is known for certain about cosmic ray origin, despite the fertile imaginations that have been brought to bear on it. The stimulus provided by this problem and by cosmic ray studies in general has led to many unexpected results of great value. Perhaps in a way it is good that a puzzle of this magnitude still remains unsolved; certainly it prevents any complacency about the completeness of our knowledge of the universe we live in. And we can be sure that, when we finally learn just where cosmic rays come from and how they acquire their characteristics, we will not only have unraveled one of the knottiest of problems but perhaps will also have discovered a fundamental secret of the universe.

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The Case for Interlingua

ALEXANDER GODE

Dr. Gode was born in Bremen, Germany, and did graduate work at Vienna, the Sorbonne, and Columbia. He came to the United States in 1927 and in 1939 was granted a Ph.D. degree in Germanics by Columbia University. He has taught modern languages at Columbia and Chicago and has been active in interlinguistics since 1934. He was editor of reference books, 1943–46, and in 1946 founded Storm Publishers, of which he is president. Dr. Gode was Director of Research, International Auxiliary Language Association, and is now Chief, Interlingua Division of Science Service.

OBSERVATIONAL STUDIES of the phenomenon "language" are of extraordinarily recent date. Just as human beings had been using their anatomy for millions of years before they got around to looking at it with critically objective curiosity (i.e., scientifically), so they were also slow in upsetting the millennial obviousness of language by asking a string of sophisticated questions about it. The scientific study of language and languages is hardly older than 150 years. What might be termed learned preoccupation with language in earlier centuries cannot be classed as science. It was at best an endeavor to describe language in terms of one specific language, that is, of course, the language of the author or speaker concerned. This is completely as though a dendrologist were to conceive of all trees as more or less surprising deviations from the apple tree in his parents' backyard (assuming that that was the only tree he knew as a child).

The prescientific attitude toward language is still very much with us. It manifests itself in the notion that language is somehow a sacred cow that must not be tampered with. Language, that is to say our language, is perfect or at least would be so if we observed all its laws faithfully, if we never split an infinitive, said "it is me," or used a preposition to end a sentence with.

As for the nontamperability of language, there is indeed a great deal to be said in support of it. There are not many instances on record where conscious interference with a linguistic situation led to a change that stuck. The word "gas" is said to have been invented by Van Helmont; the American term "blurb" was coined by Gelett Burgess; the term "bil," which the Scandinavian languages use to designate a motorcar, resulted from a prize contest organized in 1902 by a Copenhagen journal; and so forth. Such a list could surely be extended, but all its items would still remain exceptions and merely serve to emphasize that the norm is otherwise.

There is a drastic story which exemplifies at least metaphorically the fate that befalls most conscious would-be changers of language. It is found in the appendix volume to Adelbert von Chamisso's diary of the trip around the world which he took in 1815 to 1818 as the botanist attached to the Romanzow-financed expedition by Captain Otto von Kotzebue on the brig *Rurik*. Chamisso relates the story on the authority of a certain Mr. Marini who had lived on Hawaii up to about 1800. Shortly before his departure, the ruler of the island, by name of Tameiameia, found himself blessed with a son. This joyous climax to a long and grievously disappointing series of exclusively female offspring called for a special celebration. It was customary, it seems, to celebrate special events in Tameiameia's realm by outlawing some special word in the language of the island and replacing it by another of the ruler's invention. But this, Tameiameia thought, was not enough in the present case. He decided to mark the birth of his son by radically reforming the entire vocabulary of his subjects. Among the examples cited by Chamisso are the terms for man, woman, and dog, which used to be *kanaka, waheini,* and *irio* and which Tameiameia replaced by *auna, kararu,* and *japapa.* When Marini returned to the island several years later, he found to his surprise that man, woman, and dog were again *kanaka, waheini,* and *irio.* There had been an uprising of reactionary elements in the course of which all of Tameiameia's linguistic inventions and his son were killed.

For different reasons but with comparable royal intrepidity Frederick the Great of Prussia would have loved to change the German language, which he considered quite inadequate for civilized intercourse. He was especially displeased with the frequent consonantal endings of German words which lent themselves but awkwardly to composition in French Alexandrines. Frederick proposed that the situation could be remedied by at least replacing by an *e* all the *t*'s which in German (corresponding to an English *-s*) distinguish almost all third-person singulars of the present tense. Frederick had the good sense not to embody his proposal in a royal decree, and no rebellion was needed to dispose of it. But an amazing footnote to this story is that in our days the poet Stefan George, while translating into German parts of Dante's *Divina Commedia*, re-experienced Frederick's worry about the final *t* and, being a poet and not merely a king, did replace it by an *e*, at least in that one work of his.

The superstition that conscious interference with language is impossible would seem to have at least a basis in fact. But the fact is simply that *arbitrary* interference is impossible. As in grafting and cross-breeding of plants, there are definite limits to what is possible by way of consciously manipulated change, and the successful products mingle smoothly with natural growths because they are naturally possible.

However, the far-spread idea that language is above man-willed interference has its origin in the still more general superstition that language reflects somehow the laws of logic, which in turn are the laws of nature or at least their basis. Criticism of linguistic features in the name of logic is a favorite sport of the linguistically innocent. The only thing that is still more depressing is the pseudo-informed counter-assertion that language has nothing to do with logic.

To analyze this dilemma halfway satisfactorily we must accept the conceptual system underlying every human language as the distinguishing feature setting it off from all forms of expressive reactions to present stimuli which even non-human animals can communicate and comprehend. This is not the place to pursue the question whether the presence of a conceptual system that characterizes human language might not be traced to beginnings in the animal world. But we may note in passing that the most tempting positive evidence so far has been accumulated by animal-language researchers investigating communication among bees. There can be little doubt that bees do communicate with other members of their hive. What is not known as yet is whether their system of communication is inherited or learned. And this is a crucial matter, for only in the latter case would it be possible to suspect a conceptual system underlying the bee's system of communication.

It is a distinctive, and on second thought a fairly astonishing, trait of human language that it is not biologically inheritable. That a Chinese baby growing up in an American environment should not develop Chinese speech habits is too obvious to be surprising. But the important point is really that, in learning a system of English words, this Chinese baby of ours would simultaneously be acquiring and learning to manipulate a system of concepts.

Would this system of concepts have been the same if the Chinese baby had been allowed to grow up in a Chinese environment? The question may sound naive, but it is of fundamental importance.

I remember clearly when I was taught the concept vitamin. I also recall how I acquired the concept turbine. My memory is not good enough to let me remember when and how I learned the concepts cheese, sky, play, rain, snow, but I do know that I learned them all at some time in my education. The total set of concepts which I manipulate when I speak or think represents a slowly acquired thesaurus of educational coins whose exact denomination need not be paralleled in the conceptual holdings of speakers of other languages. The concept a speaker of German uses where I use "sky" covers as well what I conceive of separately as "heaven." An Eskimo cannot understand that I have a single concept "snow" to cover what for him is a variety of different things. And as I begin to look critically at my individual concepts, I am struck by such strangely arbitrary delimitations as that I should stop conceiving of rain as rain when it hits the ground and turns for me into water while I do not carry out a similar shift in the case of snow.

The arbitrary delimitation of the concepts in my conceptual system together with the knowledge that they are acquired through education and experience and can be added to through further education and experience imposes the conclusion that they are characteristic of my thinking and speaking and probably to a large extent of the thinking and speaking of other members of my speech community, but not necessarily of the thinking and speaking of a Chinese, a Tibetan, a Hopi, a Choctaw.

The world of experience may be objective; but the system of concepts into which we organize it

is a product of education, i.e., of culture. It represents a segmentation of nature which differs from culture to culture.

The functional interdependence of culture and language here envisaged has been succinctly stated by Benjamin Lee Whorf. "We cut up and organize," he wrote, "the spread and flow of events as we do largely because, through our mother tongue, we are parties to an agreement to do so, not because nature itself is segmented in exactly that way for all to see. Languages differ not only in how they build their sentences but in how they break down nature to secure the elements to put in those sentences."

If the ideas set forth in the foregoing paragraphs seem self-evident to many readers, they should acknowledge that this is so because as scientists they are constantly concerned with the formulation of new concepts and thus experience directly the fact that a concept is a mentally achieved organization of an arbitrarily circumscribed bit of reality. They are participants in a drama of revolutionary change which since about 1890 has produced in the world of science an unprecedented number of radically new concepts. As a linguist I may well wonder whether the discipline of linguistics in its recent phases could have developed its new insights and attitudes without the contemporaneous goings-on in the sciences of nature. As for the indebtedness of modern linguistics to cultural anthropology, there is no room for any doubt whatever.

The opposite view, which clings to the strict factuality of our concepts, has the backing of a century-old tradition and is by no means dead in our time. Benjamin Lee Whorf characterizes it well as upholding the absolute rule of "natural logic"—a term he uses in lieu of "common sense." Whorf then goes on: "According to natural logic, thought does not depend on grammar but on laws of reason which are supposed to be the same for all observers of the universe—to represent a rationale in the universe that can be 'found' independently by all intelligent observers, whether they speak Chinese or Choctaw. In our own culture, the formulations of mathematics and of formal logic have acquired the reputation of dealing with this order of things, i.e., with the realm and laws of pure thought. Natural logic holds that different languages are essentially parallel methods for expressing this one-and-the-same rationale of thought and, hence, differ really in but minor ways which may seem important only because they are seen at close range. It holds that mathematics, symbolic logic, philosophy, and so on, are systems contrasting with language which deal directly with this realm of thought, not that they are themselves specialized extensions of language."

In a nutshell, then, the clashing views hold on the one hand that our conceptual systems are culture-determined and variable and on the other that there is an absolute hierarchy of concepts which is identical for all and a deviation from which must be branded an error of logic.

It is doubtless correct to speak of the absolute hierarchy of concepts as the traditional notion and to claim that the notion of culture-determined concepts is a matter of twentieth-century emancipation. Their comparative importance is clearly reflected in the history of auxiliary-language theory. A sketch of this history can show that the notion of culture-determined concepts cannot trace its ancestry beyond vague beginnings in the early decades of the nineteenth century.

The fact that this same notion had been clearly formulated at least once in the thirteenth century is then of merely anecdotal importance. But this isolated curiosity is so striking that I will be forgiven for referring to it here in somewhat greater detail.

The passage in question occurs in the early chapter of Dante's *De Vulgari Eloquentia* and must be appreciated in its linguistically fascinating context of elements which the modern reader should not be too hasty in calling quaint or medieval or otherwise "scientifically unsound." "If we clearly consider," Dante wrote, "what our intention is when we speak, we shall find that it is nothing else but to unfold to others the thoughts of our own mind. Since, then, the angels have, for the purpose of manifesting their glorious thoughts, a most ready and indeed ineffable sufficiency of intellect, by which one of them is known in all respects to another . . . , they do not seem to have required the outward indication of speech. . . . The lower animals also, being guided by natural instinct alone, did not need to be provided with the power of speech, for all those of the same species have the same actions and passions; and so they are enabled by their own actions and passions to know those of others. But among those of different species not only was speech unnecessary, but it would have been altogether harmful, since there would have been no friendly intercourse between them."

Thirdly now we come to man. "Man," Dante continued, "is not moved by natural instinct but by reason, and reason itself differs in individuals in respect of discernment, judgment, and choice,

so that each one of us appears almost to rejoice in his own species." Thus "we are of the opinion that no man has knowledge of another by means of his own actions or passions, as a brute beast; nor does it happen that one man can enter into another by spiritual insight, like an angel, for the human spirit is held back by the grossness and opacity of its mortal body. It was therefore necessary that the human race should have some sign, at once rational and sensual, for the intercommunication of its thoughts, and this sign, having to receive something from the reason of one and to convey it to the reason of another, had to be rational; and since nothing can be conveyed from one reason to another except through a medium of sense, it had to be sensual."

Thus, within Dante's frame of reference, the existence of human speech is sufficiently accounted for. The immediate sequel to this discussion has no bearing on our concern. It tries to determine whether it was Adam or Eve who spoke first and concludes in favor of Adam because "it is unseemly to think that so excellent an act of the human race proceeded earlier from woman than from man." The important point is to determine what Dante meant by the assertion that "reason itself differs in individuals in respect of discernment, judgment, and choice," for in its specific context this assertion seems to imply that reason and speech are functionally interdependent.

The answer is supplied by Dante in a most original reinterpretation of the story of the confusion of tongues at Babel. To appreciate Dante's conception fully, let us remember how we conceived of the event in our earliest childhood. When after the Lord's intervention some workmen shouted "Fire! Fire!" some others understood "Feier! Feier!" and walked home because the word meant "curfew" to them.

Here now is Dante's account: "Almost the whole human race had come together to the work [of the tower of Babel]. Some were giving orders, some were acting as architects, some were building the walls, some were adjusting the masonry with rules, some were laying on the mortar with trowels, some were quarrying stone, some engaged in bringing it by sea, some by land; and different companies were engaged in different other occupations, when they were struck by such confusion from heaven, that all those who were attending to the work, using one and the same language, left off the work on being estranged by many different languages and never again came together in the same intercourse. For the same language remained to those alone who were engaged in the same kind of work; for instance, one language remained to all the architects, another to those preparing the stone; and so it happened to each group of workers. And the human race was accordingly then divided into as many different languages as there were different branches of the work."

Thus the confusion of tongues turns out to be a confusion of reason, i.e., a confusion in man's viewing the world about him. One looked at things in terms of architecture, another in terms of bricklaying; in more modern terms: one looked at the world in terms of nuclear physics, another in terms of Darwinian evolution; or in terms of Whorf's formulation: each segmented nature according to a conceptual system dictated by his education and the culture of his environment.

Supranational Languages

The history of supranational languages needs to be reviewed in terms of the dichotomy of absolute versus culture-determined conceptual systems. The problem remains well hidden in the case of all natural supranational languages. When the Greek Koiné came into vogue as an international means of communication throughout the Mediterranean basin in the Alexandrian Age, it imposed not merely its word forms on millions of speakers of the most heterogeneous languages but also its characteristic conceptual system. It had the force to do so because it owed its spread not to the consensus of heterogeneous-minded peoples but rather to a powerfully expansive cultural dynamism. The international validity of English in our time is promoted and established by a sort of cultural hegemony of the Anglo-Saxon peoples. The same holds true for the *Kuan Hua* ("speech of officials") or Mandarin Chinese, which functions as a superimposed business language in all of China with its multiplicity of diverging dialects. A simplified Malay functions in a similar way for some forty-five million people in the Malay archipelago; so do Swahili, the best-known of several African intertribal languages, and Hindustani, the "popular" speech of North India, which functions as a common tongue for some 230 million people.

All these languages impose their culturally determined conceptual patterns along with their systems of forms upon all those who find it possible to benefit practically or culturally from adopting an originally alien speech. In this they differ from colonially imposed languages only in the kind of coercion they exert.

It is easily understood that the linguist should

be particularly interested in supranational languages that owe their characteristics to the merger of influences from two or more centers of cultural radiation. This is the case in pidgin English which is (roughly characterized) a scanty stock of English word material forced into a non-European linguistic structure. A drastically simplified Italian with a strong admixture of Catalan, Spanish, French, Greek, and Arabic words originated during the Crusades in the Mediterranean countries and is still used in parts of Tunisia and Tripolitania under the name of Lingua Franca (a term which is often generically applied to all languages of the type here described).

Peculiar interest attaches to the supranational languages that serve as vehicles for religious expansion and carry with them the conceptual system of the culture in which a particular religion has its roots. In addition to the above-mentioned Greek Koiné, which came to be a religious language when its supranational standing had led most naturally to its being used as the language of the New Testament, we must list here Sanskrit and Pali of Hinduism and Buddhism and Church Slavonic and Latin of the Christian world.

The impact of Latin on the Western world is more talked about than appreciated in its implications. Its most striking characteristic is the fact that it comes in at least three clearly distinct phases. The first is the sweep of Roman colonization. Through it Latin became the language of Gaul and the Iberian peninsula, of the entire territory in which it is still spoken today in the form of the various Romance languages. But beyond these confines it affected also the language of people who emerged with linguistic autonomy from the crumbling empire: the Germans east of the Rhine and the Anglo-Saxons as conquerors of the Britons in the North.

There follows a Christian phase of the Latin hegemony over the Occident. This covers once again the lands of Imperial Roman colonization spreading the use of a Rome-centered Latin which in its ecclesiastic form becomes more and more differentiated from the vernaculars spoken in Italy, Gaul, Spain, and so forth. But in this form Latin establishes itself far beyond the realm of the once unified empire. It becomes the language of culture throughout vast regions where the language of the people is a Teutonic or Slavonic dialect. Even today the confines of this territory are visible in sharp outline marked by the range of the use of the Roman alphabet.

The third phase is the resurrection of classical Latin through the Renaissance. And this now claims our attention to an extent beyond the limits of a mere sketch.

Throughout the Middle Ages, Latin—Church Latin, that is—had been the vehicle of all learned discourse. It had been the language of European culture in all its aspects. With the Renaissance this situation underwent a radical change, and—paradoxical though it may sound—the "reclassicalization" of Latin was not the least of the factors that contributed to the collapse of Latin as the common cultural language of the Western world. Medieval Latin had been a living language. You could swear in it. You could—as one authority puts it neatly—go whoring in it without feeling that you were indulging in a display of erudition. The classical Latin of the humanists, on the other hand, was—if not dead—at any rate a resurrected language. It prided itself with being exclusive. And that at a time when participation in cultural endeavors became democratized.

If during the Middle Ages the centers of cultural endeavor had as a rule been centers of Church learning, now the middle classes in the cities had become firmly enough established to begin taking over from the clergy the function of a culturally representative class. It is a moot question whether the culturally rising middle class would have continued to use a living Latin—an organic continuity of medieval Latin—as its natural medium of intercourse, if the linguistic revolution of the Humanists had not substituted for it a superrefined instrument for exquisitely distinguished minds. The fact is that the resurrected classical Latin of the Humanists lent itself arduously to the cultural purposes of the expanding culture-bearing class.

But a third factor contributed simultaneously to the weakening of Latin's claim to being the natural language of European cultural exchange and intercourse. This is the rise of nationalism and the concomitant national pride in the excellence of the national tongue.

The centers of culture and learning which in pre-Renaissance centuries had been ecclesiastic in spirit and purpose assumed after the Renaissance a more and more secular demeanor. We may well speak of a progressive secularization of the universities, and if we consider the part played in post-Renaissance times by princely courts and middle-class-initiated academies, we are struck by the fact that for them Latin was never a natural workaday medium of expression. To put it pointedly: When Roger Bacon got up from his studies, in the pursuit of which he naturally used Latin as the normal

linguistic instrument, he used the same Latin to order his famulus to bring him something to eat. When, on the other hand, Sir Isaac Newton interrupted the composition of his Latin *Principia*, it was roast beef and not *caro bubula tosta* he called for.

The progressive recession of Latin as the common cultural language of the Western world set the stage for the earliest conscious preoccupation with the problem of a universal auxiliary language. The most important works here to be cited are Leibnitz' *De arte combinatoria* of 1666 and John Wilkins' *An Essay Toward a Real Character and a Philosophical Language* of 1668. It is the approach these thinkers pursued that interests us particularly. In the *Nouveaux Essais*, which Leibnitz wrote in 1704, there is a chapter heading, "De l'imperfection des mots." This is the keynote to his lifelong preoccupation with a reformed symbolism of thought. Latin had been used for centuries as a symbolism of thought. Leibnitz mastered this symbolism. He also mastered French and likewise German. But what he was after was a *perfect* symbolic system which would imply a perfect method of thought. Back in 1629 Descartes had expressed this same concern in a letter to Père Mersenne in which he referred to a work on Chinese ideographs and their possible use in a universal system of communication. "J'oserois esperer ensuite," Descartes wrote, "une langue universelle fort aisée à apprendre, à prononcer et à écrire, et, ce qui est le principal, qui ayderoit au jugement, luy representant si distinctement toutes choses, qu'il luy seroit presque impossible de se tromper."

"A language apt to serve our judgment because it can represent things so distinctly that it will become almost impossible for our judgment to err," that is what Leibnitz and his contemporaries wished to achieve, for languages like Latin, French, and German—all the traditional symbolisms of thought—seemed to hinder rather than help clear thinking. The ideal language of the searcher after truth was a system very much like mathematics: it could almost actively produce truth if only its user knew how to manipulate its symbols correctly.

The faith here implied in the absolute existence of a hierarchy of ideas which can think and lead to truth almost without the intervention of man and certainly without the intervention of a nonconceptually and emotionally contaminated language, this faith seems to have a strange fascination. It can be encountered even today. Even now at the rate of two or three a year idealistic crackpots produce outlines (never more than outlines) of universal tables of concepts and corresponding symbols which (when completed) can be manipulated as universal systems of communication with foolproof elimination of all possibilities of error or misunderstanding.

In a less extreme form the Leibnitzian faith in an absolute conceptual system has influenced all serious interlinguistic endeavor throughout the nineteenth century and beyond. The contrasting view, which holds that concepts are forms, given by the psychic entity of speech-thought and used by us to organize our perceptional field, was formulated for the first time in 1808 by Friedrich Immanuel Niethammer in his work *Über Pasigraphie und Ideographik*, apparently in pursuance of a suggestion made by Friedrich August Wolf. It was extremely slow in gaining recognition, but the trend of which it is symptomatic led at least to the predominant acceptance of the principle that an auxiliary language might profitably be built after the pattern of living tongues. This is apparent in Schleyer's construction of Volapük in 1880 and still more in Zamenhof's Esperanto of 1887. But all these projects have in common the endeavor to improve on the "imperfection" of traditional tongues by evolving a systematic hierarchy both of forms and of concepts. And they attempt and claim to be as universal as Leibnitz' *ars combinatoria* hoped to be.

What is completely and radically new in projects of supranational communication of the type of Volapük and Esperanto is their motive. Especially the latter is explicitly designed to eradicate misunderstandings in human intercourse. This has its roots in early experiences of the Polish-Russian-Jewish Zamenhof. As a child in his native town he had been struck by the observation that German, Russian, Polish, and Yiddish-speaking groups often came to blows because they could not understand each other. This local situation, he felt, was a perfect illustration of conditions in the world at large. If people could understand each other, there would be neither beerhouse brawls nor wars. So far it is impossible to disagree with Zamenhof. But, when the argument goes on to claim that people would understand each other if they spoke the same language, one cannot help having some guarded doubts. Language, after all, is as useful for purposes of camouflage and worse as it is for laying bare one's innermost soul.

There is something nostalgically beautiful about the naïveté of the true Esperantist's faith in the power of his language. One may be justified in suspecting that it is this faith in its often fanatical

excesses that explains why Esperanto has progressed as far as it did, and not really the power of the language itself.

The grammatical system of Esperanto is both a simplification and an ingenious extension of the accustomed categories of the European languages. The vocabulary consists of a carefully chosen list of roots which are manipulated with a strictly schematic set of affixes. A dog is *hundo,* a bitch *hundino,* a puppy *hundido,* a she-puppy *hundidino,* a big dog *hundego,* a big bitch *hundinego,* a big puppy *hundidego,* a big she-puppy *hundidinego,* etc. *Preĝi* is to pray, *mi preĝas* I pray, *mi preĝis* I prayed, *mi preĝos* I shall pray, *mi preĝus* I should pray, *preĝu* pray!, *preĝejo* church, and so on.

The concepts covered by the roots are those Zamenhof felt subjectively to be indispensable. The forms were subjectively picked by him with a certain awareness of the fact that those represented in several natural languages were to be preferred. In cases of uncertainty he chose words from languages not as yet sufficiently represented to make the entire project impartially just. So boy came to be represented by *knabo* (after German *Knabe*), ship was *ŝipo* (straight from English).

The mastery of this system gives the adept a feeling of prestidigitatorial pride. It works! It is amazing what you can do with a little skill! But the fun is spoiled when one tries to build up conceptual complexes like automobile, orthography, elephantiasis, excrescence.

Until one runs across conceptual problems of the kind just enumerated, one is tempted to consider Zamenhof's system an answer to Leibnitz' prayer. But Leibnitz wanted a full and exhaustive pyramid of concepts and corresponding symbols, whereas Zamenhof suddenly veers away from the consequences of his logical approach as soon as the results promise to be hermetic. He supplies at this point the famous rule XV, which I quote from his *Fundamenta Krestomatio* (p. 254): "La tiel nomataj vortoj fremdaj, t.e., tiuj, kiujn la plimulto de la lingvoj prenis el unu fonto, estas uzataj en la lingvo Esperanto sen ŝanĝo, ricevante nur la ortografion de tiu ĉi lingvo; sed ĉe diversaj vortoj de unu radiko estas pli bone uzi senŝanĝe nur la vorton fundamentan kaj la ceterajn formi el tiu ĉi lasta laŭ la reguloj de la lingvo Esperanto." Or in English: "The so-called foreign words, that is, those which the majority of languages have taken from one source, are used in the Esperanto language without change, receiving only the orthography of the latter language; but with various words from one root it is better to use unchanged only the fundamental word, and to form the rest from this latter in accordance with the rules of the Esperanto language."

I need not analyze further the strange ambivalence of Esperanto. Suffice it to summarize that in this language the rationalistic ideal of a logical hierarchy of concepts bows to the sweep of modern international words. For this should be noted: the expression "foreign words," which from an English point of view seems rather obscure, is taken directly from the German *Fremdwort* and applies to, among other things, all the modern scientific and technological terms that speakers of all sorts of languages concoct out of Greek and Latin material.

In so far as Esperanto embodies what Zamenhof called the *vortoj fremdaj,* it assumes traits of a common European language and abrogates its own oft-repeated claim to universality, for in practice the *plimulto de la lingvoj,* "the majority of languages," must not be taken literally but allowed to coincide with the majority of the languages of the Western world.

It is not as a universal language but as in part a common European tongue that Esperanto has been able to develop a fairly strong appeal in the Orient, especially in China and still more in Japan, where it has proved its ability to function as a language of science and technology, provided both the author and reader are initiates of its cult. In this we may see a realistic supplement to the basically emotional motivation of Esperanto promotion and dissemination.

Yet this question of motivation, which introduced and concluded the foregoing references to Esperanto, needs to be asked with much more incisive brutality. It is clear what inspired Leibnitz and his contemporaries in their quest for a universal medium of thought. It is likewise clear how languages like medieval Latin, Swahili, pidgin English, and the rest came to be languages of international communication. They were needed to carry, and in turn were carried by, some expansive force that could not be contained within the limits of one nation. They were functionally dependent upon some form of colonial, commercial, or religious imperialism. Latin was first the language of conquering legions, then that of an ever-militant Church. Swahili, which originated as the hybrid tongue of the descendents of Arab-Negro intermarriages on the Somali coast, spread to the region of the Great Lakes in answer to trade needs of Arab caravans and later into the Congo basin through the pressure of European colonial and

commercial influences. On the other hand, a great language like Hebrew never achieved extranational currency because until recently it had survived exclusively as the language of a strangely unexpansive, non-missionary religion.

On this basis the whole question might be summarized in these terms: An international language can subsist only as the speech instrument of an expansive supranational dynamism of which it is both vanguard and adequate expression. It is not likely that an artificially constructed language could ever fulfill these requirements, but this last point may well be left out of consideration, for the real issue is quite directly whether or not a supranational dynamism is at work in a given place and at a given time.

If there is nothing of the sort in our day and age, then all our preoccupation with an international language is a harmless and futureless pastime. If on the other hand a cultural power of international scope does exist, then it is not you or I who can decide what sort of an international language we should like to have for it. That cultural power itself will do the deciding, and all we can do is clarify and accelerate the process.

We speak often and insistently of this world of ours as *one*. We take for granted the powers that made it one in so far as it is so. Our religions, our philosophies, our faiths and ideologies are multifarious as of yore. They make us the individuals, the partisans, the nationals we are. They do not make us citizens of the one world. That is the doing for better or worse of that huge event in the current phase of human history which we call by the somewhat niggardly name of science.

The justification of this assertion, its illustration by way of tracing the history of science and its worldwide drive, is a link in the chain of our argument which we may consider established. It is likewise an established fact that science—whatever its branches, reflections, antecedents—is a growth of post-Renaissance Western European history.

The concepts of science have emerged in Western thought and fit the patterns of the corresponding languages. No one has formulated this observation more strikingly than Benjamin Lee Whorf. "The world view of modern science," Whorf wrote, "arises by higher specialization of the basic grammar of the western Indo-European languages. Science of course was not caused by this grammar; it was simply colored by it. It appeared in this group of languages because of a train of historical events that stimulated commerce, measurement, manufacture, and technical invention in a quarter of the world where these languages were dominant." And further: "Among our modern European languages, with perhaps Latin and Greek thrown in for good measure, there is a unanimity of major pattern which exists only because these tongues are all Indo-European dialects cut to the same basic plan, being historically transmitted from what was long ago one speech community; because the modern dialects have long shared in building up a common culture; and because much of this culture, on the more intellectual side, is derived from the linguistic backgrounds of Latin and Greek. From this condition follows the unanimity of description of the world in the community of modern scientists. But it must be emphasized that 'all modern Indo-European-speaking observers' is not the same thing as 'all observers.' That modern Chinese or Turkish scientists describe the world in the same terms as Western scientists means, of course, only that they have taken over bodily the entire Western system of rationalizations, not that they have corroborated that system from their native posts of observation."

Conceptually the language of science is Western European. And, since it is the language of science which alone in our time is truly an international language, Western European is *the* international language of the twentieth century.

What is conceptually correct is likely to be formally correct. And, indeed, not only the concepts of science but also its formal terminologies are Western European. This does not mean that the technical terms of science are derived from this or that Western European language. The point is precisely that they are not French or English or German or Latin or Greek. The word "statistics" is *not* German, even though a German was the first to use it. "Penicillin" is *not* English; "automobile" is *not* French. Nor are these terms Greek or Latin. They are products of the modern world albeit of traditional stuff.

European Language

The existence of a common European language is a correlate of the common occidental civilization. On the level of anthropological linguistics the keenest observations on this count have again been made by Benjamin Lee Whorf, and it is he to whom we owe the term "Standard Average European." In a somewhat specialized context, Whorf wrote as follows: "A comparison between Hopi and western European languages makes evident that the grammar of Hopi bears a relation to Hopi culture, and the grammar of European tongues to

our 'Western' or 'European' culture. And it appears that the interrelation brings in those large subsummations of experience by language, such as our own terms 'time,' 'space,' 'substance,' and 'matter.' Since with respect to the traits compared there is little difference between English, French, German, or other European languages with the possible exception of Balto-Slavic, I lump these languages into one group called SAE or 'Standard Average European'."

If we propose to view, let us say, Spanish and Portuguese as dialects of one and the same standard language which happens not to have been codified, no one will object or find the idea outlandish with the exception of a half dozen exceptionally stubborn Portuguese linguochauvinists. To link Sardinian and Italian, Provençal and French, or even all the Romance languages as variants of a common norm may likewise be accepted as a matter of course. If it is borne in mind that English has a vocabulary of predominantly Romance origin, the postulation of a homogeneous Anglo-Romance group of languages turns out to be fairly easy too. In this latter step, one may have to give a special accounting of the little sentence builders—words like I, to, over, not, will—which are clearly Anglo-Saxon and are rarely replaceable by more highfalutin doublets of Romance origin. Yet on the whole these words perform functions comparable to those of their opposite numbers in Romance, and, again on the whole, they correspond fairly well in an item-by-item way to Romance words.

In what sense, however, would it be possible to draw a language like German into the orbit of SAE? In an earlier passage we have tried to speak of the strong impact of Latin on German and other languages akin to it. German does have a large number of terms coming directly from Latin, and it also has a tremendous learned vocabulary which appears in the language of specialists under international forms.

Yet the most incisive phase of what we may call the historical Europeanization of German has not yet been touched upon. Medieval ecclesiasticism brought into German lands not just a restricted number of terms but rather a whole world of concepts. These were new concepts which established a basic pattern for German thought and made of it a strand in the tradition of occidental, Western, Mediterranean, or European thought. The number of common European concepts thus represented in German is not a whit smaller than the number of such concepts found in English, or French, or Spanish for that matter. Yet, since there was no native population descended from Imperial Roman settlers and no equivalent of the Norman conquest, the introduction of concepts like "conscience," "purgatory," "Saviour," or "cancer," "prejudice," "minority," was not managed by straight terminological adoption. The terms were analyzed in their meaning as derivatives, compounds, or similes, and corresponding German words were built of corresponding raw materials. In this way it came to pass that the concept of "conscience" is represented in German by a term that really says "con-scientia," that is, a collective knowing (*Gewissen*); "purgatory" is really a place where one gets purged or swept (*Fegefeuer*); "influence" is really flowing in (*Einfluss*); something "superfluous" is flowing over (*überflüssig*); "suicide" is self-murder (*Selbstmord*); "equator" equaler (*Gleicher*); "cancer" a crab (*Krebs*); "milliped" a creature with a thousand feet (*Tausendfüssler*); "to possess" to besit (*besitzen*); and so forth.

The number of these terms is tremendous. Yet it is not their number alone that makes them important. What matters particularly is the fact that these terms are derivationally clear and concrete. To illustrate: If the Romans had used a term like *influentia* in the extended sense of our "influence," they would instinctively have felt that the notion was one of flowing in. They could have played on the word, rejecting, for instance, the statement that somebody was under the influence of somebody else by suggesting that the situation was rather the other way round, that it was not *influentia* but *effluentia*. A German can do that with *Einfluss-Ausfluss* exactly after the Latin pattern, while those languages which have not only the Roman notion but also the Roman form could follow suit at best in a very artificial and cumbersome procedure. Only a sophisticated college graduate can understand me if I insist that there was no influence but rather a flowing out. And not even he will laugh.

Indeed, the man who first claimed that German was the most Romance of all Romance languages must have done so tongue in cheek, but he knew what he was talking about.

The Language of Science

The "language of science," as we have amply demonstrated, is common European. But is it really correct to speak of the vast stock of internationally current technical terms as a language? It is perhaps a highly characteristic point that science has

carried to all parts of the globe its system of concepts and its corresponding terminologies but not its complete and coherent language. The reason is that it actually has nothing of the sort, a fact which may seem to many a tempting invitation to find here symptom, symbol, or proof of non-existence at this time of a full and coherent world view of science.

We cling to clearer evidence by insisting at this point that the international terminologies of science, which admittedly have not coagulated in one complete and coherent international language, do form an integral part of SAE. In this setting they do represent a complete and coherent language. And, if in their international functions they are to develop or be developed into a full-fledged language, there is no other system but SAE in which they can thrive and be naturally effective.

The idea that common European is destined to serve as *the* international language of modern times is not new. It was vaguely in Niethammer's mind when he wrote his above-mentioned *Pasigraphie*. It must have been sensed by Schleyer and Zamenhof, who took the bulk of their raw material from occidental sources. But the first to present it in fairly tangible terms was the German-Chilean surgeon Liptay, who more than half a century ago stated, in a preliminary sketch of what he was then planning to elaborate into a complete auxiliary language, that the only originality of his plan was that it was devoid of originality, that the production of an international language was not required since one existed—latent in the common elements of the various national tongues—and that it had merely to be discovered, not invented.

Since Liptay's time, the idea has had a slow and arduous growth. The most pronounced clarification was contributed by Edgar de Wahl in his auxiliary language, Occidental, of 1922. But full emancipation from the last vestiges of conceptual absolutism, of full internationality achieved through logic rather than in the wake of facts and trends in history, has not been attained before the advent of Interlingua in its present form.

Under the sponsorship of Science Service, the linguistic system Interlingua is making a strong bid to serve as the medium of international communication in science, technology, and other fields in which both the initiative and the objectives are such that a restriction of their audience to the limited range of individual national tongues is paradoxical and hence inefficient. Interlingua proposes to succeed where Volapük, Esperanto, and a host of other schemes have failed. Its claims, therefore, need to be examined critically in the widest context of implications.

The vocabulary of Interlingua embodies all the word material that the languages of the Western world have typically in common. It manipulates this material according to a system of word compounding and derivation obtained from the same sources. It operates with a minimum set of grammatical features all of which its source languages are unanimous in requiring as indispensable devices in daily intercourse.

The research underlying Interlingua was a matter of practical observation, never of theoretical experimentation. None of its elements reflects merely what its compilers, in subjective rationale, considered "handy" or "nice to have." Strictly speaking, the language is not *constructed* but *extracted*. It aims to be simultaneously French, English, Spanish, Italian, and so on, each one of these languages streamlined by the elimination of its idiosyncratically distinctive features in such a way that the result coincides with the corresponding streamlining product of the others.

Interlingua looks thus: *"Sub le gerentia de* Science Service, *le systema linguistic interlingua presenta con urgentia su candidatura a servir como medio de communication international in le scientias, le technologia, e altere campos in que tanto le initiativa como etiam le objectivos es tal que un restriction de lor auditores al confinios del linguas national individual es paradoxe e consequentemente inefficiente. Interlingua promitte succeder ubi volapük, esperanto, e un hoste de altere schemas ha fallite. Pro iste ration su pretensiones debe esser examinate criticamente in le plus large contexto de implicationes."*

The major theorems with which Interlingua stands or falls are the following:

1. The languages of the world are not reducible to one universal pattern. There is no "absolute" linguistic pattern, and every language—natural or constructed—falls of necessity into one of many possible specialized molds.

2. The global internationalism of the twentieth century is propelled by ideological forces of occidental origin. These are conceivable only in occidental thought (and speech) molds; they are so conceived by all participants regardless of what their accidental native language background may be.

3. The languages of the Occident are—by origin or in consequence of historical influences or both—so closely akin to one another that it is both sound

and illuminating to conceive of them as variants of one type language which has fittingly been called Standard Average European (SAE).

4. The natural and only possible supranational language of science—which, in the most comprehensive sense, coincides with the forces of contemporary internationalism—is Standard Average European. Of this Interlingua is to date the most satisfactory formulation. Eventual improvements of it, such as are characteristic of all living languages, will come in the form of organic developments through usage.

The methodology used in the elaboration of Interlingua was consciously designed to yield a codification of the existing common European language. The result is a living language, not a dead artifact. Hence Interlingua can change and grow. It can be developed further through usage. It can even be reformed if someone proves that this or that feature of its underlying methodology did not yield as perfect an approximation of the common Western type as it could and should have. But change and growth and development and reform cannot replace Interlingua by something strikingly different as long as science with its occidental "grammar" controls the common progress of mankind.

And a final word: Let us respect the non-scientific cultures by recognizing that their "grammars" may differ from that of science and by insisting that the expansive dynamism of Interlingua as an adjunct to that of science shall not violate their prerogatives in their natural habitat.

Whorf wrote: "I believe that those who envision a future world speaking only one tongue, whether English, German, Russian, or any other, hold a misguided ideal and would do the evolution of the human mind the greatest disservice. Western culture has made, through language, a provisional analysis of reality and, without correctives, holds resolutely to that analysis as final. The only correctives lie in all those other tongues which by aeons of independent evolution have arrived at different, but equally logical, provisional analyses."

Interlingua corresponds to the provisional analysis achieved in Western thought. It unifies the dialects of the Western world, not the languages of the world. Its internationality is as vast and as limited as that of science, in which the provisional analysis achieved by Western thought is given a provisional justification.

Note: The reference to Chamisso is *Werke*, Bong, V, 57. All the Whorf quotations are from *Four Articles on Metalinguistics*, Foreign Service Institute, Department of State, Washington, D. C., 1950. The basic Interlingua manuals are *Interlingua-English, a Dictionary of the International Language*, prepared by the Research Staff of the International Auxiliary Language Association under the Direction of Alexander Gode, 480 pages; and *Interlingua, a Grammar of the International Language*, prepared by Alexander Gode and Hugh E. Blair of the Research Staff of the International Auxiliary Language Association, 128 pages, both Storm Publishers, New York, 1951. Both antecedents and methodology of Interlingua are accounted for in considerable detail in the prefatory apparatus of *IED* (*Interlingua-English Dictionary*).

Southern College and University Faculty Research Resources in Physical Science and Engineering Fields

HEATH K. RIGGS

The author was born in Richmond, Vermont, and was educated at the University of Vermont where he taught mathematics and later became Director of Admissions. He holds a Ph.D. degree in higher education from the University of Chicago and since 1951 has been Chief of Educational Research, Southern Regional Education Board, Atlanta, Georgia.

LAST October* John I. Mattill reported to the readers of THE SCIENTIFIC MONTHLY on a national inventory of college and university research resources completed in 1951 by the Engineering College Research Council. The data of this inventory has now been further analyzed by the Southern Regional Education Board to find out how Southern colleges and universities stand in terms of faculty research potential in physical science and engineering fields, to what extent this potential is active in research, and how much of this is active in defense research. The basic purpose was to locate by fields and by states (although the relative state findings are not reported here) the qualified but unused research potential of Southern faculty. The findings of the Mattill article, combined with the findings of this study, afford an opportunity to make some interesting regional and national comparisons. These findings take on added significance, since according to the report, "substantially all" the national potential for research in colleges and universities in the physical sciences and engineering was reported.

It is hoped that the general method employed in this study of drawing from national data and findings, findings of value to particular regions, will encourage and assist other regions to make similar analyses.

Of the 24,881 faculty members of the three levels of professorial rank and full-time research personnel in the nation in all physical science and engineering fields, 5,121 (21 per cent) were in the colleges and universities of the Southern Compact region.† Of the 21,037 in the nation considered qualified by their institutions to do research, 4,155 (20 per cent) were on Southern faculties. Of the 12,866 engaged in research in the nation, 2,321 (18 per cent) were in the South. These are equivalent to 6,745 full-time research workers in the nation and 1,183 (17.5 per cent) in the South. Finally, of the 3,291 in the nation doing research for national defense in February, 1951, 437 (13.3 per cent) were on Southern faculties.

From these findings it may be concluded that Southern colleges and universities were doing in February, 1951, (in terms of time spent) between one-fifth and one-sixth of the college and university

* John I. Mattill, "College and University Research Resources," THE SCIENTIFIC MONTHLY, Vol. LXXV, October 1952.

† The Southern Compact region is composed of the following fourteen states joined together by an interstate compact for the improvement of graduate and professional education: Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, and Virginia.

research of the nation in these fields but only between one-seventh and one-eighth of the research for national defense. As of February, 1951, Southern colleges and universities were not receiving a proportion of defense research equal to their proportion of the nation's faculty and full-time research personnel in physical sciences and engineering fields, or equal to their proportion of the nation's faculty members considered qualified to do research, or equal to their proportion of the nation's active research men in colleges and universities, or equal to their proportion of the nation's total faculty time spent on research.

Furthermore, about seventeen hundred Southern faculty members considered qualified by their institutions to do research in nineteen physical science and engineering fields remained without research projects. They represent considerable human resources available to meet research manpower needs. But it is even more valuable to know the fields in which this qualified but unused research potential is located. In Table 1 such fields are identified. Those fields with the greatest relative research reserve are listed first. Less than half of the qualified Southern faculty and full-time research personnel in astronomy, marine engineering, mechanics, industrial engineering, mechanical engineering, metallurgical engineering, mathematics, electrical engineering, civil and sanitary engineering, and mining engineering were active in research in February, 1951. On the other hand, more than two-thirds of the qualified faculty of the South had research projects in ceramics and glass, chemical engineering, earth sciences, chemistry, and food technology fields.

TABLE 1

RELATIVE USE OF RESEARCH RESOURCES IN THE SOUTH

Field	Percentage of qualified faculty active in research
Astronomy	32
Marine engineering	36
Mechanics	37
Industrial engineering	38
Mechanical engineering	41
Metallurgical engineering	42
Mathematics	45
Electrical engineering	45
Civil and sanitary engineering	46
Mining engineering	47
Electronics	55
Petroleum and fuels engineering	55
Aeronautical engineering	60
Physics	63
Food technology	69
Chemistry	70
Earth sciences	71
Chemical engineering	77
Ceramics and glass	84

TABLE 2

RELATIVE DEFENSE USE OF RESEARCH RESOURCES

Field	Percentage of total research time on defense research		
	South	U.S.	Difference
Civil and sanitary engineering	20	76	56
Mechanical engineering	21	48	27
Mining engineering	0	25	25
Mathematics	14	37	23
Electronics	64	81	17
Petroleum and fuels engineering	0	17	17
Industrial engineering	1	18	17
Earth sciences	23	38	15
Astronomy	24	37	13
Ceramics and glass	27	39	12
Mechanics	50	56	6
Chemistry	32	38	6
Physics	65	70	5
Aeronautical engineering	78	82	4
Food technology	19	19	0
Chemical engineering	38	36	−2
Electrical engineering	82	76	−6
Metallurgical engineering	82	70	−12
Marine engineering	86	71	−15

It may also be of some assistance to federal agencies who make research grants and contracts with colleges and universities to list the fields in which Southern faculties spent a smaller proportion of their total research time on defense research in the field than the national average. Table 2 gives such information. It appears that in the first fourteen fields listed in Table 2 Southern colleges and universities were doing less defense research in proportion to their total research in the field than the national average. Combining the findings of the two tables, it may be noted that from one-third to two-thirds of the qualified faculty of the South are without research projects in eleven of these same fourteen fields. On the other hand, Southern faculties are doing more defense research in proportion to their total research in marine engineering, metallurgical engineering, electrical engineering, and chemical engineering than the national average for these fields.

In two other respects the national findings as reported in Mattill's article are also true for the Southern Compact region. The Research Council Committee found "that colleges and universities are maintaining emphasis on the fundamental sciences." More than 51 per cent in the South and 53.5 per cent in the nation of all faculty research time in physical sciences and engineering fields was devoted to the basic sciences of chemistry, physics, mathematics, earth sciences, and astronomy. Similarly, 49.4 per cent in the South and 50.2 per cent in the nation of all research time spent on defense projects was in the same basic sciences.

Finally, about the same concentration of defense research holds for three universities in the South as holds for fifteen universities in the nation. Three Southern colleges and universities account for 47 per cent of the total faculty time spent on defense research in all Southern colleges and universities, whereas these same institutions account for only 6.5 per cent of the total faculty and senior research staff members. In these three schools, 72.5 per cent of the faculty are active in research, and 71.5 per cent of their research time is devoted to defense research. Similarly, the Mattill article reported:

> The survey figures show that fifteen colleges and universities account for one-half of the total faculty time spent on defense research throughout the nation, whereas these same institutions account for only 20.5 per cent of the total faculty and senior research staff members. In these fifteen "favored" schools, 75 per cent of the faculty are active in research and 70 per cent of their research time is devoted to defense research.‡

Other comparisons made in the Mattill article for the fifteen institutions in the nation and for the three in the South, and likewise for the 498 other educational institutions in the nation and for approximately 127 in the South responsible for the other half of all defense research time, follow similarly.

These findings on the distribution of defense research provide a possible explanation as to why educators today take such opposing views as to the effect of sponsored research on the educational programs of the institutions. Educational leaders have reacted differently to sponsored research because such research has affected their institutions differently. At some institutions the volume of sponsored research has been so great that educational leaders of these institutions have justly been concerned that teaching programs may have been crowded into the background and that the institutions themselves may have become dependent upon federal support to a considerable extent. At the same time, other institutions in which little sponsored research has taken place, have, to a greater degree, tended to recognize the advantages of sponsored research in expanding the total volume of research on their campuses, in enabling them to retain their graduate faculty while enrollment was relatively light, and in contributing to the advanced education of students by providing research experience and financial support. These differing views of the effect of sponsored research are perhaps merely recognizing the more basic fact that there has been a rather uneven distribution of defense research in colleges and universities in relation to the existing distribution of qualified human research resources.

Both the Mattill study and this one indicate that the critical shortage of technically trained research manpower is to some extent an artificial shortage brought about by the uneven distribution of research funds in relation to the distribution of qualified human research resources. If the nation is to get a maximum benefit from available qualified human research resources, and if the nation is to meet the critical shortage of research talent that federal defense agencies claim, without endangering the supply of new science graduates, additional methods will have to be developed by colleges and universities, and also by federal and state agencies and industries, which will encourage and insure expanded and broadened research opportunities in all sections of the nation for qualified personnel.

What kinds of methods might be explored? Some of the following possibilities might merit consideration.

1) A systematic assessment of the research potential and research use of the nation's colleges and universities in all fields of knowledge (not simply the physical science and engineering fields).

2) The development of more effective ways of keeping institutions informed of the research needs and interests of state and federal agencies, and agencies of the qualifications and interests of faculty research personnel and of institutions to do specific types of research.

3) Studies to develop a practical guide as to the amount and kinds of research a college or university can safely assume to maximize the advantages and minimize the dangers of sponsored research.

4) The development of methods to stimulate qualified faculty members to assume more initiative in developing research ideas and in presenting these in the form of proposals to agencies, foundations, and industries for research support.

5) Self-evaluation by colleges and universities to determine the kinds of basic research the institutions are qualified to do, taking into account personnel qualifications and interests, facilities for research, and unique environmental conditions.

6) Interinstitutional cooperation to work out ways by which the administrative "know how" of institutions with considerable sponsored research experience may be made available to other institutions with little research experience and to consider subcontracting large projects among groups of institutions.

7) A review of the research that federal agencies

‡ The Scientific Monthly, Vol. LXXV, p. 236, October 1952.

are doing in their own laboratories to determine whether some of the basic research might more appropriately be conducted through grants or contracts with colleges and universities, where students may be trained at the same time.

8) The development of administrative methods in federal agencies which will actually produce a decentralization of research contracts and grants.

9) Studies of the kinds of basic research industry would be willing to support through grants.

10) Explorations between state universities and the various agencies or departments of state government as to the types of research appropriate to universities, which the state needs for continued development.

As colleges and universities, federal and state agencies and departments, and industries learn to work together developing additional methods, of which these are only illustrations, perhaps the nation can approach the goal of maximum benefit from all its qualified human research resources and eliminate such artificial shortages as may exist.

POSITION IS EVERYTHING

At the beginning of the Time-Scale men and apes are hardly distinguishable. The Erect-Ape-Men appear to have neither articulate speech, nor traditions, nor accumulated knowledge. They have few means of defense or aggression except the physical force of their bodies and the instinctive aptitudes provided by their biological inheritance. To us they appear to be associating and contending with the apes on fairly equal terms. But if we turn to the end of the Time-Scale (April 23, 1935), we can see at once that something has happened: nothing to the apes, but something to man. The apes look and behave at the end of the Time-Scale very much as they did at the beginning: during 506,000 years they have repeated their activities instead of extending them. But man at the end of the Time-Scale is not what he was at the beginning. He no longer contends and associates with his cousins the apes. He puts them in the Zoo. And if, some fine morning, he should encounter his ancestor *Pithecanthropus* on University Avenue, he would no doubt, failing to recognize the old man, put him in the Zoo also. Imagine then some descendant of *Pithecanthropus* standing in the Zoo, looking up at his remote ancestor in the cage. In what essential respects do they appear to differ? As biological specimens they appear to us not too unlike; and if the man finds the antics of *Pithecanthropus* amusing, it is chiefly because they parody his own on the less formal occasions of life. If the man should be suddenly whisked to the beginning of the Time-Scale and dropped, naked and without appliances, among his ancestors of Java, the amusement, if any, would not be his. All the biological process of 506,000 years would lead him swiftly to the final, good or bad, end of extinction. Fortunately for the man, he is not at the beginning but at the end of the Time-Scale, in the Zoo, looking up at *Pithecanthropus* with amusement. The reason he can afford to be amused is a simple one: he is on the Outside of the cage, *Pithecanthropus* is on the inside.*—*Progress and Power*, CARL BECKER

* Carl Becker, *Progress and Power*. Copyright 1935 by Alfred A. Knopf, New York, and reproduced with the publisher's permission.

Reconnaissance Studies of the Green River*

H. E. THOMAS†

The author is Staff Geologist for the Ground Water Branch of the U.S. Geological Survey and has held this office since July, 1952. He received his Ph.D. in geology from the University of Chicago and taught geology at Ohio State University before joining the Survey in 1929. He was district geologist in charge of ground-water investigations of the Salt Lake City, Utah, district from 1939–49, except for a period of service as Lieutenant-Colonel, Corps of Engineers, during World War II. He is the author of The Conservation of Ground Water *and is an outstanding authority in this field.*

CIVILIZATION has long since engulfed the West, and most of the hardships and romantic adventure that characterized the "Wild West" of the nineteenth century now exist only in movies, television, western fiction, and the nostalgic reminiscences of old timers. But there has been very little change in the canyons through which the Colorado River and its principal tributary, the Green River, traverse numerous mountain ranges and plateaus. In Utah these rivers are accessible in very few places to the man with a car. Their tortuous courses can be followed only by boat, and the trip is about as hazardous as when Major John W. Powell first made it in 1869. An outboard motor will now take a boat rapidly through the numerous stretches of smooth water, but the boat arrives sooner at the next swirling, rock-studded, and treacherous rapid, where manual dexterity alone wards off disaster. A trip down these rivers is an outstanding adventure, and one that has been experienced by very few, because numerous tragedies over the years have proved the folly of attempting it without the help of experienced rivermen.

Several expeditions through the canyons have been made by engineers and geologists of the U. S. Geological Survey. From these expeditions have come plans and profiles of the Green River, notes on the geology[1] and studies of utilization and flood control of the river.[2] In recent years Survey engineers, notably LaPhene Harris and the late Norman Nevills, have been among the best navigators of white water in the West (Fig. 1).

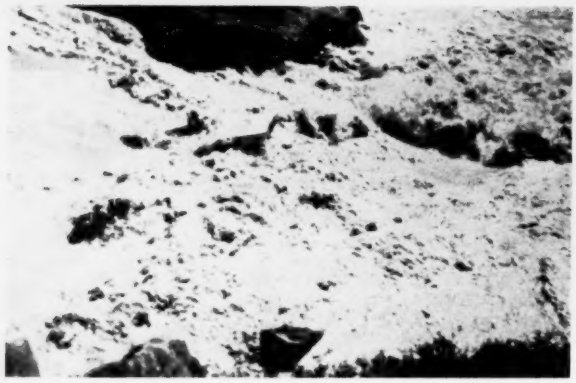
Purpose of the Boat Reconnaissances

The waters of the Colorado River basin are of major concern to seven Western states, and echoes of the arguments as to who shall benefit from the development of those waters have doubtless been heard in all parts of the nation. Our knowledge of the water available for development in the basin is based almost entirely upon records obtained at a network of gaging stations, each of which provides continuous records as to the quantity of water passing the station, and some of which provide records also of the suspended sediment, the dissolved materials carried by the water, or both (Fig. 2). Comparison of records from successive gaging stations along a stream shows the net effect of all gains to or losses from the stream in the intervening reach: gains by tributary inflow, ground-water inflow, and precipitation upon the water surface; and losses by diversions, ground-water outflow, evaporation from the water surface, and transpiration of riparian vegetation. If the hydrology of the region had been studied in sufficient detail, it would be possible to discriminate these various gains or losses in stream flow.

Unfortunately, not enough is known about the Colorado River basin to explain the vagaries of discharge of the river or its principal tributaries. This is particularly true of the Upper Basin, which is the 110,000-square-mile drainage area above Lee Ferry, Arizona. Detailed studies to show the occurrence of ground water and its relation to

* Publication authorized by the Director, U. S. Geological Survey.

† Staff Geologist, Ground Water Branch, Water Resources Division, U. S. Geological Survey, Salt Lake City, Utah.



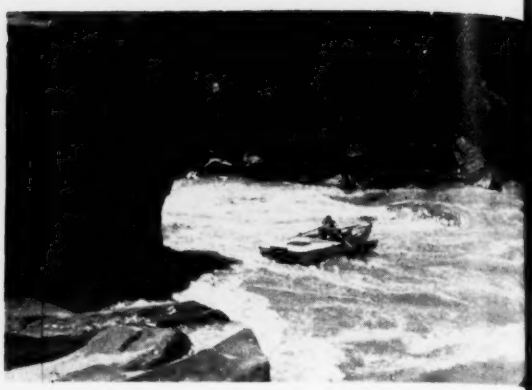


FIG. 1. Ashley Falls, in Red Canyon of the Green River. *Left*, June 1947; LaPhene Harris, navigator. *Right*, Septe ber 1947; Harold Chase, navigator.

stream flow, the natural discharge of water by evapotranspiration, the water-bearing properties of the rocks of the basin, and the relation of the rock strata to the dissolved minerals and suspended sediment in the streams, have not been made for any part of the Upper Basin.

Far more basic hydrologic data than are now available will be required for administration of the Upper Colorado River Basin Compact of October 11, 1948. The basis for apportionment of water is contained in Article III of the compact, and Article VI states:

> The Upper Colorado River Commission shall determine the quantity of the consumptive use of water, which use is apportioned by Article III hereof, for the Upper Basin and for each State of the Upper Basin by the inflow-outflow method in terms of man-made depletions of the virgin flow at Lee Ferry, unless the Commission, by unanimous action, shall adopt a different method of determination.

It is evident that proper apportionment of the water in accordance with the compact will require information as complete as possible concerning the quantities of water, both surface and subsurface, that cross state boundaries, and the relation of those quantities to the flow in the Colorado River at Lee Ferry. With respect to each project for development of the water resources in the Upper Basin, the compact requires a determination of the "quantity of consumptive use of water . . . in terms of man-made depletions of the virgin flow at Lee Ferry," a determination that requires knowledge of the quantities diverted for the project and the quantities returned to the stream, as well as the difference between natural losses before and after the project begins operation. Inasmuch as the consumptive use of water by each state is calculated in relation to its depletion of the virgin flow at Lee Ferry, it becomes essential to know the extent of the natural losses from the riv before these virgin-flow conditions are changed the development of projects.

In so large an area as the Upper Colorado Riv basin, complete information regarding sources a movements of water, measurement of water cro ing state boundaries, and determination of natur losses from the river system will require large e penditures and a large corps of hydrologists. It likely, however, that investigation of the ma stems of the Colorado and its principal tributar will show that some parts of the drainage bas make negligible contributions to the stream flo and that study of those areas can be deferred. I Utah those main stems are in deep canyo throughout most of their courses, and not easi accessible for study. The best method of coverag of the entire course of the river is by boat. Som of the information obtained during boat trips— for instance, the data concerning the relation o stream flow to the regional geology and ground water hydrology—is considered to be pertinent t the hydrology of the basin at all seasons. Measure ments of stream discharge and of the mineral con stituents in selected water samples, however, pro vide only fleeting glimpses of the continually changing conditions in the basin. Important clue to the hydrologic relationships of the basin may b derived by analysis of these reconnaissance data in relation to the continuing records from estab lished gaging stations along the Colorado River and its tributaries.

The Green River in Utah and Colorado

The Green River (Fig. 3) barely crosses the state line from Wyoming into Utah before it plunges into a succession of canyons, including the Flaming Gorge, Horseshoe, and Kingfisher Canyons, and then turns eastward through Red Can-

to skirt the highest part of the towering Uinta ountains. In Colorado it flows lazily through the tively open Browns Park, then turns southward ough Lodore Canyon to Echo Park, and then k into Utah through Whirlpool Canyon. After other short calm stretch in Island Park, it unces down through the sharp arch of Split ountain before emerging in the broad Uinta sin near Jensen, Utah. There for several miles ranches here and there along its banks, which n be reached by road in several places. Then the er traverses Desolation Canyon and Gray Can- n before emerging briefly in the broad Gunnison alley around Greenriver, Utah. The lower 95 iles of the river's course is in Labyrinth Canyon nd Stillwater Canyon. Its confluence with the olorado River is practically inaccessible except y boat.

Four reconnaissance trips have been made by oat down these canyons of the Green River for e purpose of measuring all tributary inflow and etermining the discharge of the river at numerous ctions not included in the gaging station network. hese boat trips were made by the Water Re- ources Division of the Geological Survey, under e direction of M. T. Wilson, district engineer in alt Lake City. Three were made in September nd October of 1946, 1947, and 1948, when the ischarges of the river and tributaries were at or ear the minimum for the year. One trip was ade in June, 1947, during the period of maxi- um discharge from melting snow.

These reconnaissance trips provided valuable ata as to the tributary inflow and other hydrologic actors. The data collected in the 1948 reconnais- ance provided a better opportunity to evaluate the ffect of evapotranspiration losses and ground- water gains upon the inflow of the stream, for the ollowing reasons: gaging stations recently estab- lished on the Green River near Jensen and near Ouray in the Uinta Basin permitted analysis of the discharge at those points throughout the period of the reconnaissance; the discharge of the river in September, 1948, was lower than at any time since 1940, and the gains and losses by ground water were accordingly a larger proportion of the flow; and because of favorable meteorological conditions there was very little storm runoff dur- ing the 1948 reconnaissance, in contrast particu- larly to the condition during the 1946 trip, when storms created a flood wave with peak discharge more than double the minimum flow of the pre- ceding week.

The 1948 reconnaissance was made during the period September 14 to 29. The program for this reconnaissance included, as for previous trips, the measurement of the main-stem discharge at numerous sections and the determination of all tributary inflow. In addition, observations were made as to ground-water inflow, losses by evaporation and transpiration, and the geologic conditions that might be expected to affect the stream flow. The geology and ground-water hydrology of the Green River along the 439-mile channel from the Wyoming state line to the confluence with the Colorado River have been summarized elsewhere.[3]

Stream Gains and Losses During Boat Trip 1948

In 1948 the minimum discharge of the Green River was recorded at Linwood, Utah (near the Wyoming state line) on September 17, and at Greenriver, Utah, about a week later. Thus the boat trip of September 14–29 coincided approxi-

FIG. 2. Gaging station on the Green River at Jensen, Utah. During low stages, stream velocities and cross-sectional areas are determined by wading measurements using the Price current meter. Cable car is used for determining concentration of suspended sediment, using D-43 sampler, and for measuring discharge at higher stages. Continuous record of stage is maintained by recording gage, in shelter on bank.

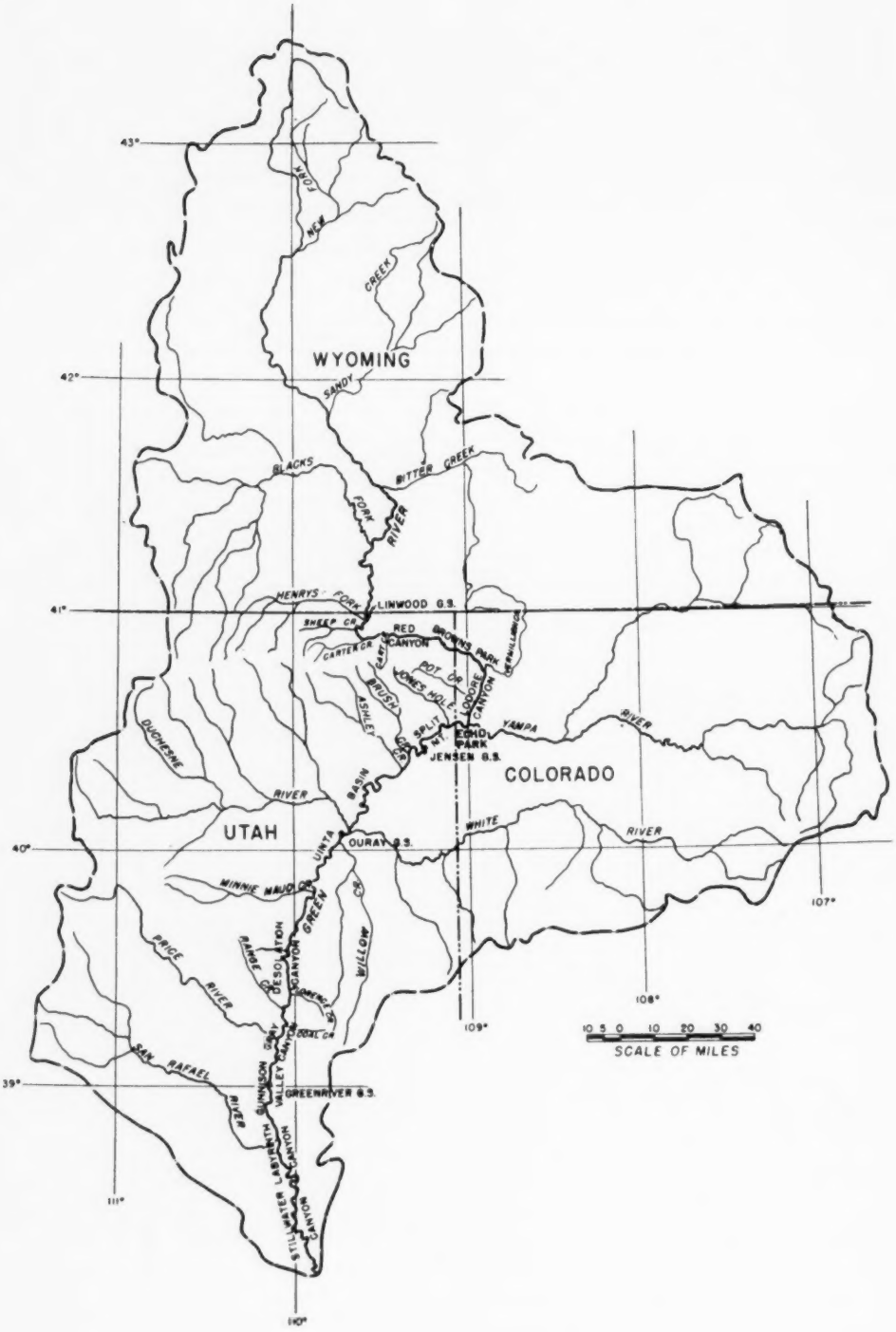


FIG. 3. Map of Green River basin.

mately with the period of minimum flow for the year. Quantitative estimates of the gains or losses due to ground water or evapotranspiration can be made for this period of minimum flow, provided that channel and bank storage at the beginning and end of the designated period are approximately equivalent. The period selected for analysis is therefore one in which the river stage at the beginning and at the end is approximately the same and is not subject to rapid change. The selected period progresses downstream with the approximate velocity of the river flow and is thus about a week later at Greenriver than at Linwood.

The average discharge of the river during the selected 21-day base period was computed from the records at the four gaging stations to be 515 cubic feet per second (cfs) at Linwood, 678 cfs at Jensen, 1,027 cfs at Ouray, and 1,074 cfs at Greenriver. On the basis of measurements during the boat trips and comparison with the gaging station records, the discharge of the Green River at its mouth was estimated to average about 974 cfs during the base period.

Inflow from Tributaries. The principal tributary inflow to the Green River during the 1948 boat trip was from the White River (340 cfs), Yampa River (94 cfs), Jones Hole Creek (30 cfs), Duchesne River (22 cfs), Carter Creek (20 cfs), and Minnie Maud Creek (12 cfs). Gaging stations on the White, Yampa, and Duchesne Rivers several miles above their mouths provided records which could be used in estimating the average flow in the 21-day period, and adjustments were made also for some of the smaller tributaries. The total tributary inflow to the Green River in Utah and Colorado in the 21-day base period was computed to be 560 cfs, of which about 350 cfs came from the White and Duchesne rivers, and about 175 cfs from Yampa River and minor tributaries, such as Gorge Creek, between Linwood and Jensen (Fig. 4).

Gain from Precipitation. During the 21-day base period rain was recorded at every Weather Bureau station in the Green River basin. During the storm of September 16–19, 1948, it exceeded 0.25 inch in the Uinta Range and headwater areas of the Green and Yampa Rivers, and reached a maximum of 0.94 inch near the northern tip of the basin. The storm of September 25–30 covered only the southern half of the basin; the recorded rain was less than 0.25 inch except in the headwaters of the Yampa, where it reached a maximum of 1.61 inches.

These storms had a readily detectable effect upon runoff. The precipitation in the northern part of the basin during the first storm caused an increase in flow at Linwood three days after the rain had ceased, and at Jensen two days later. The high areas in northeastern Utah also received enough precipitation during this period to cause storm runoff at Jensen and Ouray as early as September 19. The storm of September 25–30, which missed the northern part of the basin, caused no storm runoff at Linwood and Jensen but resulted in slightly increased flow at Ouray and later at Greenriver.

FIG. 4. Measuring the flow of Gorge Creek into the Green River in Red Canyon, September 1947.

The total storm flow during the base period is small, and well within the limits of error of the stream-discharge determinations. It is equivalent to an average during the base period of 8 cfs at Linwood, 12 cfs at Jensen, 25 cfs at Ouray, and 35 cfs at Greenriver. Thus, the inferred storm runoff was about 1.5 per cent of the total flow at Linwood and increased to 3 per cent of the total flow at Greenriver.

Loss by Evaporation. The area of water surface is shown on topographic maps at scale 1: 31,680 prepared during river surveys in 1904, 1914, and 1922. In September 1948 the river discharge was less than in corresponding months in those years, and the river level was undoubtedly lower than at the time of the surveys. The width of the river is arbitrarily assumed to have been 10 per cent less in canyons and 20 per cent less in other reaches than shown on the topographic maps. Thus computed, the water surface of the Green River in Utah and Colorado in September 1948 was about 19,000 acres, including more than 13,000 acres of smooth shallow water in Browns Park, Uinta Basin, and the meandering reaches of the river below Greenriver.

The rates of evaporation from water surfaces have been estimated from records of U. S. Weather

Bureau evaporation stations at Fort Duchesne and Vernal, Utah. Several investigators[4,5] have reported that the evaporation from a Weather Bureau class A land pan is about 50 per cent greater than that from a reservoir surface, and Follansbee[6] accordingly has multiplied the land-pan evaporation by a factor of 0.69 to derive the evaporation from reservoirs. Sleight found also that evaporation from slowly flowing water was about 7 per cent greater than that from still water, but he did not determine quantitatively the relation of stream velocity to evaporation. In the absence of experimental data to show the rate of evaporation from turbulent flow such as is encountered in the rapids of the Green River, it is assumed that evaporation from smoothly flowing water is equivalent to 85 per cent of the land-pan evaporation, and that evaporation from turbulent water is equal to the evaporation as recorded at land pans. These factors are approximately equivalent to those used by the Bureau of Reclamation, in unpublished notes, to compute the evaporation from the river surface. No allowance has been made for the moderate to strong upstream winds which are characteristic of the canyons, and which would increase the evaporation opportunity above that of the Weather Bureau evaporation stations, where average wind velocities may be less.

The evaporation from the river surface during the base period in 1948 is computed to have been at an average rate of 150 cfs, of which about 10 cfs was lost in Browns Park, 45 cfs in Uinta Basin, and 70 cfs in the Gunnison Valley and lower reaches of the river. According to these computations, evaporation was at an average monthly rate of about 5¾ inches from the water-surface area of the Green River in Utah and Colorado.

Loss by Diversion. Small quantities of water are diverted from the Green River in Utah and Colorado for irrigation of adjacent flood plains. The largest of these diversions is in Gunnison Valley near the town of Greenriver, where diversions may be as much as 80 cfs during the irrigation season. The quantities of water diverted have not been measured, nor have the irrigated areas been determined. Instead, the irrigated areas have been included in the total area of evapotranspiration losses, and the diversions are thus included among the natural losses from the river.

Loss by Evapotranspiration. The river doubtless loses some water by evaporation from moist banks and by transpiration of vegetation, especially willows and saltcedars that line the banks in many places. Except in the canyons the river is bordered by flood plains, which are covered in most places by water-loving vegetation such as willow, saltcedar, cottonwood, tule, marsh grass and salt grass. These plants draw ground water from under the flood plain. It is likely that some of the water used by these plants in September 194 was not replaced until the river reached a higher stage—perhaps not until the 1949 freshet. However, water was observed seeping into the river from bank storage at many places during the 194 boat trip; this ground-water seepage to the river might well have been greater but for the transpiration of flood-plain vegetation.

Gaging station records show diurnal fluctuations in river discharge during September 194 amounting to as much as 12 cfs at Linwood and 18 cfs at Ouray in the Uinta Basin. These diurnal fluctuations afford a minimum measure of the total evapotranspiration draft from river surface and flood plains. A large proportion of the water lost by evapotranspiration may be derived from tributaries and from ground water moving in the alluvium toward the river. Particularly in the Uinta Basin, it is likely that the water so lost comes chiefly from the basin area rather than from the main stem of the river. Nevertheless, the total draft of evapotranspiration in the channel and flood plain of the river represents a depletion from the river of water that would otherwise continue downstream.

The total area of flood plain bordering the water surface of the Green River in Utah and Colorado was about 41,000 acres in September 1948; this total included 20,700 acres in the Uinta Basin, 6,700 in Browns Park, and 6,400 in Gunnison Valley.

The rates at which water is used by the flood-plain vegetation cannot be computed accurately from available data. Studies throughout the West have indicated that willow, cottonwood, and salt grass use water at rates of 60 to 85 per cent of the evaporation from a standard Weather Bureau class A land pan; saltcedar, tules, and cattails use considerably more, but greasewood uses less. The amount and density of cover by individual types of plants have not been mapped, however, and the depth to the water table, which is an important factor in the rates of transpiration by these water-loving plants, is not known in any part of the flood plain.

In order to arrive at an estimate of the total flood-plain evapotranspiration, it is assumed that the average rate was 80 per cent of the recorded evaporation from the nearest Weather Bureau

land pan. By this assumption, the evapotranspiration during September 1948 from the 41,000 acres of flood plain was at an average rate of 3 1/3 inches, which resulted in a water loss at the rate of 280 cfs. This rate is somewhat greater than that computed on the basis of Thornthwaite's[8] "potential evapotranspiration," and somewhat less than that computed on the basis of Blaney and Criddle's[9] "water requirements" for medium to dense native vegetation.

Computed Inflow of Ground Water. The observed ground-water inflow to the Green River totalled 12 cfs during the 1948 boat trip, of which about half came from warm springs in Split Mountain Canyon. In many other places the geologic formations and structure appeared to be favorable for contributions of ground water to the river channel, even though such contributions could not be observed.

The total ground-water inflow during the 21-day base period in 1948 may be computed as the difference between the sum of the gains by inflow from Wyoming and from tributaries, and the losses by discharge into the Colorado River, water-surface evaporation, and flood-plain evapotranspiration.

The boat reconnaissance also provided data as to the relative amounts of ground-water inflow in various segments of the river. In at least two areas, analysis of these data and of the stream discharge as recorded at numerous gaging stations shows the desirability for additional hydrologic research.

Uinta Basin. About 166,000 acres of land in the Uinta Basin are irrigated by diversions from tributaries of the Green River, including especially the Duchesne River. The inflow to the basin is measured at gaging stations above these diversions, but the gaging stations near the mouths of the tributaries and below the irrigated areas measure only a part of the outflow: in September 1948 the combined discharge of the Duchesne River, Ashley Creek, and Brush Creek was only 25 cfs, but the computed ground-water flow to the Green River flood plain within the Uinta Basin was about 240 cfs. This ground-water inflow could not be discriminated in the records of discharge at Greenriver (below the Uinta Basin), for that discharge in September 1948 was only about 30 cfs greater than the discharge at Jensen (where the river enters the Basin) plus the inflow from the White and Duchesne Rivers and minor tributaries. Thus it is evident that the evapotranspiration losses between the gaging stations were nearly as great as the computed ground-water inflow.

GAINS AND LOSSES IN GREEN RIVER, SEPTEMBER 1948

Gains, cfs		Losses, cfs	
Inflow from Wyoming	515	Discharge to Colorado River	974
Tributary inflow	560	Water-surface evaporation	150
	1,075	Floor-plain evapotranspiration	280
Computed ground-water inflow	330		1,404
	1,405		

Of the computed ground-water inflow of 330 cfs, 65 cfs was contributed to the river in the 123-mile segment between Linwood and Jensen; 243 cfs in the 133-mile segment within the Uinta Basin; and only 22 cfs in the lower 180 miles of river channel.

Conclusions

Information collected during the period of minimum discharge in 1948 indicates that the water in the Green River at the Wyoming state line (515 cfs) would have been reduced about 85 per cent by evapotranspiration losses of 430 cfs in Utah and Colorado, if there had been no inflow. The inflow in those States, however, totalled about 890 cfs, of which 560 cfs was contributed by tributaries and about 330 cfs by ground water, and the flow of the river into the Colorado in this period of minimum discharge was about 974 cfs.

Of course, the unmeasured ground-water inflow and evapotranspiration loss are larger in relation to the stream flow during periods of minimum discharge than at other times. Nevertheless, an accurate evaluation of the consumptive use within the Uinta Basin by the inflow-outflow method "in terms of man-made depletions of the virgin flow at Lee Ferry" (as required by compact) will require more hydrologic data than can be provided by the present network of stream-gaging stations. The Uinta Basin is the principal area of ground-water inflow to the Green River, and may well be the largest contributor of ground water in the entire Colorado River Basin. The quantitative determination of this contribution is possible with adequate hydrologic data, and these quantities can be checked against the gains and losses in stream flow

FIG. 5. Triplet Falls in Lodore Canyon, above Echo Park.

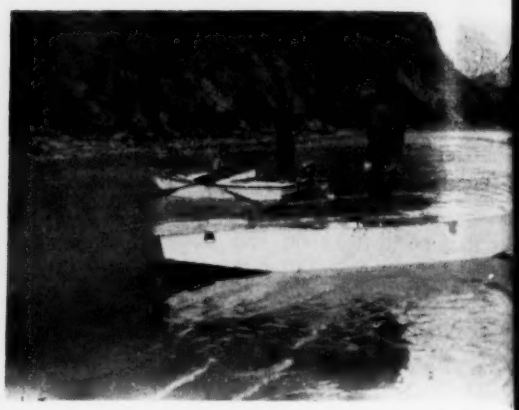


FIG. 6. Quiet water in Whirlpool Canyon below Ech Park.

and the beneficial consumptive use and other evapotranspiration loss within the basin.

Proposed Echo Park Reservoir. The proposed Echo Park Reservoir, on the Green River in northwestern Colorado (Figs. 5 and 6), is admirably suited to storage of water with minimum losses by evaporation. However, part of the reservoir area is underlain by cavernous Mississippian limestone, one of the most permeable formations in the region. As shown by records in September 1948, there are no measurable losses into these limestones at low stage. Between the gaging stations at Linwood and Jensen the computed evapotranspiration losses at that time were at an average rate of about 80 cfs, and there must have been about 65 cfs of groundwater inflow.

The Green and Yampa Rivers *do* lose water at flood stage as they cross the Uinta uplift—that is, between the Wyoming state line and Jensen on the Green, and below Cross Mountain on the Yampa.[10] These losses range from 50,000 to 80,000 acre-feet during May, June, and July, and are probably due in part to replacement of water taken by evapotranspiration from some 18,000 acres of flood-plain areas, especially in Browns Park, Island Park, and Lily Park. Gaging stations are not numerous enough to discriminate the losses, if any, by seepage into the limestone. Geologic conditions do not appear conducive to major losses from the reservoir by seepage into the limestone, but no guarantee can yet be made against such losses, because large areas underlain by the Mississippia limestone have not been mapped topographicall or geologically.

An adequate accounting for these losses fro the Green and Yampa Rivers during flood stag would require detailed studies in areas where lar evapotranspiration losses are suspected—notabl Browns Park—and studies of the geology and hy drology of the area south of the reservoir site, t determine whether any losses from the propose reservoir may occur by leakage into the limeston

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On Absolute Measurement*

N. ERNEST DORSEY† AND CHURCHILL EISENHART‡

Dr. Dorsey did both his undergraduate and his graduate work at The Johns Hopkins University, receiving his A.B. in 1893 and his Ph.D. in Physics in 1897. After teaching at The Johns Hopkins unitl 1901, he joined the U S. Department of Agriculture. In 1903 he went to the Bureau of Standards as an assistant Physicist, later becoming Physicist until his retirement in 1943. He is a former associate editor of International Critical Tables, *and the author of numerous scientific papers and books. Dr. Eisenhart has served as Chief of the Statistical Engineering Laboratory of the National Bureau of Standards since 1946. He became a member of the faculty of the University of Wisconsin in 1937. During World War II he was engaged in mathematical research on aerial gunnery and rockets. Dr. Eisenhart is the author of more than 50 technical papers and is one of the editors and authors of the book* Selected Techniques of Statistical Analysis.

BY an absolute measurement of a physical quantity, such as the velocity of light, is meant the determination of the value of that quantity in terms of the significant fundamental units of length, mass, time, etc., and of those constant parameters that characterize the accepted system of theoretical equations that connect the several pertinent quantities. (p. 9.)

Theory of Errors

The mean of a family of measurements—of a number of measurements of a given quantity carried out by the same apparatus, procedure, and observer—approaches a definite value as the number of measurements is indefinitely increased. Otherwise, they could not properly be called measurements of a given quantity. In the theory of errors, this limiting mean is frequently called the "true" value, although it bears no necessary relation to the true quaesitum, to the actual value of the quantity that the observer desires to measure. This has often confused the unwary. Let us call it the limiting mean.

Let e denote the amount by which a given member of the family departs from the limiting mean, and let e_q denote that value which in the indefinitely extended family is surpassed by half of the e's; that is, it is an even chance that a given member of such an extended family departs from the limiting mean by as much as e_q.

The quantity e_q, the quartile error, commonly called the probable error of a single observation, will in this study be called the technical probable error of a single member of such a family. (p. 4.)

It should be noticed that the technical probable error either of a single measurement or of the mean of a group of n measurements indicates merely the closeness with which that measurement or mean probably approaches the limiting mean. It tells nothing whatever about the actual quaesitum, and so it is of very minor interest to the experimental physicist engaged in absolute measurements.

To him its main interest is threefold: (*a*) It tells him when it has become profitless to take additional routine observations; but in most cases other and more important considerations set another limit. (*b*) It may enable him to state positively that a systematic error affects one or both of two rival families of measurements. (*c*) It, as applied to a relatively small number of observations, enables him to state positively that systematic errors smaller than a certain amount cannot with certainty be detected experimentally with the apparatus and procedures employed in obtaining those measurements.

The last is, for him, by far the most valuable property of the technical probable error. But in practice he seldom thinks of it in that connection. By what seems to be a kind of intuition, he recognizes rough numerical relations between the mini-

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† Physicist (retired), National Bureau of Standards. Associate editor, *International Critical Tables*, 1922–1929.

‡ Chief, Statistical Engineering Laboratory, National Bureau of Standards.

mum detectable error and the mean deviation of the several determinations from their mean. And he studies those deviations without thinking about the technical probable error. Actually, the relations he uses are practically those that may be derived in the following manner from the technical probable error.

The argument runs as follows: If the means of two groups of measurements do not differ by at least the sum of their technical probable errors, then the existing difference is not sufficient to justify the assumption that they do not belong to the same statistical family. Consequently, if the only basic difference between the groups were the presence in one of a systematic error that was absent from the other, then the presence of that error could not be certainly established from the difference, unless it amounted to at least the sum of the two technical probable errors. Conversely, it cannot be proved that the measurements are not affected by such an error. (pp. 5 and 6.)

. . . the term "systematic error" is used to cover all those errors which cannot be regarded as fortuitous, as partaking of the nature of chance. They are characteristics of the system involved in the work; they may arise from errors in theory or in standards, from imperfections in the apparatus or in the observer, from false assumptions, etc. To them, the statistical theory of errors does not apply. They are frequently called "constant errors," and very often they are constant throughout a given set of determinations, but such constancy need not obtain. For example, if the value found by a certain measurement depends upon the humidity of the air, which the experimenter fails to record, thinking that it is of no consequence, then the measures will be affected by a systematic error which will, in general, vary throughout the day and especially from day to day. (p. 6.)

Averaging

Any set of numbers may be weighted as desired, and summed and averaged, and the result can be carried out to as many digits as one may wish. The procedures are simple, exact, and not open to any question or criticism. They are purely arithmetical.

But if the numbers represent physical quantities, then questions arise concerning both the validity of averaging and the number of digits that have a physical significance.

1) It is sometimes forgotten that the averaging of a set of values, even of the same kind, may be a physically invalid procedure. That is, that the average may not deserve greater confidence as an estimate of the quaesitum than do the individual values.

For example, consider a series of sets of determinations, each set being affected by a systematic error peculiar to it; that error being constant throughout any given set, but varying from set to set. Superposed on that error are fluctuating errors of various kinds. These last are minimized, set by set, by averaging the determinations composing a set. This averaging is entirely proper. But it leaves one with a series of values that differ, one from another, on account of the presence of systematic errors peculiar to each. In general, the averaging of such a series of values will be quite invalid; in general, the average will not deserve more confidence than do the individual values. The only cases in which it will be justifiable when the values differ by more than can be accounted for by the irregularities inherent in each of the several sets, are three: those in which it is definitely known—or perhaps is very highly probable—that the variation in the systematic error from one value to another either is (*a*) strictly fortuitous, in which case the fluctuating part of the error is minimized by the averaging, or (*b*) arises from the error fluctuating between equal and fixed positive and negative values, the number of positive values being essentially equal to the number of negative ones, or (*c*) arises from the error varying progressively from a positive value to a negative one as certain uncontrolled conditions change, and those conditions are known to vary in such a way that each negative error will in the long run be matched by an equal positive one.

Only when one knows a great deal about the systematic error can one be sure that any of these conditions are satisfied. And when he knows that much, he can often arrange to eliminate, or to evaluate, the error; and he should do so.

The cases that most frequently give trouble are those in which the data give evidence of the presence of a systematic error, but the experimenter does not know its source, and those in which another studying the data finds evidence of a systematic error that was overlooked by the experimenter. In such cases one may not know how the error varies with the conditions. If it makes all the values too great, then the smaller ones will be better than the average. Or the reverse may be true. Or the error may be present in some and absent from others; then averaging will not improve things.

Under such conditions it is quite improper to present the average as being superior to the individual values.

One is never justified in merely guessing that averaging will minimize or eliminate the effect of a systematic error. He must know it, must know that under the actually existing conditions the error is so minimized or eliminated.

In the absence of such knowledge, the proper brief summation of the work would seem to consist in a giving of the extreme values with a statement that at least some of the values seem to be affected by a systematic error of unknown origin. To this might well be added the experimenter's opinion, and if he wishes, the arithmetical average, with a clear statement of its questionable value. To give merely the average tends to mislead the reader, to blind him to the presence of systematic errors. The reader must always be on guard, as it is not very uncommon for a writer to average his results quite invalidly, either because he has not awaked to the fact that averaging may be invalid or because he has failed to recognize the evidence for the existence of systematic error.

2) The number of digits that are of physical significance in the sum and in the average must be carefully considered. (pp. 6 and 7.)

Quaesitum

The quaesitum of the investigation is the actual value of the quantity. The particular value yielded by a given apparatus, procedure, and observer is of no interest in itself, but only in connection with such a study as will enable one to say with some certainty that the value so found does not depart from the quaesitum by more than a certain stated amount. No investigation can establish a unique value for the quaesitum, but merely a range of values centered upon a unique value. The quaesitum may lie anywhere within that range, but the wiser and more careful the experimenter's search for systematic errors, and the more completely he has eliminated them, the less likely is it to lie near the limits of the range. The wider the range, the less becomes the physical significance of the particular value on which the range is centered. (p. 9.)

Definitive Value

The term "definitive value" is used in two distinct, though related, senses. (*a*) In a narrower, particular sense, it denotes the value that is believed to lie as near the quaesitum as any that can be legitimately derived from the observations taken in the course of the work being reported. It is the ultimate or definitive value to which that work itself leads. It is often called the "final" value of the work. (*b*) In a broader, general sense, it denotes the value that is believed to lie as near the quaesitum as any that can be derived from a consideration of all the determinations that have been made, and of all other available pertinent information. Whenever not otherwise indicated by the context or a modifier, it is in this broader sense that the term is to be understood.

Every report of measurements of a physical quantity should state clearly the particular definitive value to which those measurements lead. It may also give the broader definitive value based on everything that is known. But the two should not be confused, as unfortunately they often are. (p. 9.)

Dubiety

The determination of the range is of an importance that is secondary only to that of its center. No absolute measurement has been completed until values have been established for both of those quantities. The determination of the range necessarily involves an element of judgment, and the limits cannot be set with precision. Nevertheless, it is possible to assign a lower limit; and although no fixed upper limit can be assigned, it is possible to say that if suitable care and diligence had been employed, it is not likely that the range exceeds a certain specified value.

In order to distinguish this range from the numerous kinds of "errors" that abound, its half will in this study be called the "dubiety" of the value found. If that value be denoted by V, and the dubiety by D, then the quaesitum will likely lie within the range $(V - D)$ to $(V + D)$. By this, one means that nothing has come to light in the course of the work to indicate that the quaesitum lies outside that range.

The dubiety is made up of three distinct additive terms to which it is convenient to give descriptive names. They are as follows:

Mensural dubiety arises from the uncertainties in the several primary measurements and in the elimination of known systematic errors. It is common practice to take the arithmetical sum of the effects of these individual uncertainties as an upper limit for the mensural dubiety.

Discordance dubiety arises from the fact that the discordance in the individual determinations limits the smallness of a systematic error that can be experimentally detected. The result cannot be less dubious than the size of the largest systematic error that can escape detection. This term of the dubiety is generally the most important by far, and the least understood and least appreciated by those who are not experimentalists.

Deficiency dubiety arises from the determinations

being too few; in particular, finite in number. It is equal to the technical probable error of the result. This term, much honored by those not skilled in experimentation, is always smaller than the discordance dubiety and frequently is negligible in comparison therewith.

Of these three terms, the second alone needs to be especially considered here. In searching for systematic errors, the logical procedure is to make a series of measurements, then to change something and to make another series, and to compare the means of the two groups. This will be repeated as often as may seem necessary. None of the series can be long, for an extended delay offers opportunity for unanticipated changes to occur. If the two means being compared do not differ by more than the sum of their technical probable errors, their difference is of no physical significance—it proves nothing. Hence, the presence of a systematic error that does not exceed the sum of the technical probable errors of the two groups of observations used in the search cannot be established without great difficulty, if at all. That sets a minimum limit for the discordance dubiety. (pp. 9 and 10.)

Obviously, no one should claim a discordance dubiety that is smaller than the smallest systematic error that he might certainly have detected by the tests he made. But there may be reasons that seem to him sound for believing that the actual dubiety is smaller than that. In such case he may, and generally should, state his belief and the reasons therefor; but the statement should never be of such a kind as to lead the reader to confuse the writer's estimate with the minimum discordance dubiety as just defined. (p. 10.)

But on comparing a series of determinations made by different persons with significantly different apparatus and procedures, it may be found that the several members of the series agree more closely than their individual dubieties would lead one to expect. Then if the differences in apparatus and procedure are sufficiently fundamental, one might be justified in thinking it very improbable that the quaesitum lies far outside the range of the means of the several members of the series. And from the whole he might infer a smaller range of possible values than that demanded by the dubieties of the several determinations. (p. 10.)

No one is really interested in how near the quaesitum the definitive value may possibly lie, for he knows that by chance the two may coincide even though the work be very poorly done. But one does keenly desire to know how far the two are likely to differ—how dubious the definitive value may be. And it is the plain duty of the experimenter not merely to show that his definitive value may be that of the quaesitum, but to prove that it is unlikely to depart from the quaesitum by more than a certain stated amount. In order to obtain the information needed to meet that demand, the careful experienced investigator will proceed somewhat as follows. (p. 10.)

Procedure

Before one undertakes an absolute measurement in physics, he will make a careful theoretical study of the problem, including, among other things, methods of attack, sources of errors and how they can be avoided or eliminated, and types of computation. On the basis of that study, the apparatus will be constructed and set up. Only then does the investigation itself begin.

Working standards of the absolute units required must be carefully compared with primary standards. This will ordinarily be done at some standardizing laboratory, which will certify those working standards as being correct under certain specified conditions to within, say, a in 10^n. That value is accepted by the experimenter and sets the top limit to the known accuracy attainable in the work. If, for example, the absolute measurement attempted were simply a length, and the working standard were certified as correct to 3 in 10^5, then the absolute measurement (which determines merely the ratio of the measured length to that of the working standard) could under no condition give the value of the quaesitum to a known accuracy that exceeds 3 in 10^5. No matter how small the technical probable error of the measurements might be, the dubiety of the result cannot be less than 3 in 10^5. Indeed, the dubiety of the value found for the quaesitum will in general be distinctly greater than that, on account of errors inherent in the absolute measurement itself.

The experimenter will measure each of the involved quantities in terms of the appropriate working standard, taking pains to observe as well as may be the conditions laid down by the standardizing laboratory, and to determine carefully whatever is necessary to correct for the actual deviations from those conditions. He will do this repeatedly, and he will also measure them under deliberately different conditions, so as to obtain a check on the accuracy with which he can correct for departures from the specified conditions.

Having found that the apparatus seems to be

working properly, he will change, one by one, and by known amounts, each of the adjustments, and will note how each change affects his observations. If possible, he will carry each maladjustment to a point where it produces an easily measurable change in his observations; and if maladjustments in both directions (positive and negative) are possible, he will similarly study each. Thus he will find how important the several adjustments are, the accuracy with which they must be made, and perhaps how to detect each maladjustment experimentally and to correct for the error that it produces.

Readjusting the apparatus, he will proceed to change, one by one, every condition he can think of that seems by any chance likely to affect his result, and some that do not, in every case pushing the change well beyond any that seems at all likely to occur accidentally.

There still remains the possibility of systematic errors arising from unsuspected causes, from secular variation in laboratory conditions (temperature, humidity, light, vibration, etc.), possibly from solar, lunar, or atmospheric effects, etc. So the observer will take long series of observations, extending over weeks, months, or years, noting carefully everything that seems either pertinent in itself or of assistance in fixing the attendant conditions. These will be worked up, day by day, carefully compared with one another, and probably plotted in such a way as to show clearly any change that might appear. From time to time changes will appear, and will be studied.

Thus the experimenter presently will feel justified in saying that he feels, or believes, or is of the opinion, that his own work indicates that the quaesitum does not depart from his own definitive value by more than so-and-so, meaning thereby, since he makes no claim to omniscience, that he has found no reason for believing that the departure exceeds that amount.

That is exactly what he means. He does not mean as some have suggested, that he is of the opinion that the chances are only one in a hundred, or in a thousand, or in some other number n, that the quaesitum's departure from his definitive value exceeds that amount. He, differing from those others, feels that it would be foolish for him to make such a statement, that it could be nothing more than a gambler's guess. For how can one say, without stultifying himself, that the chance is one in n that the error produced in his result by an entirely unknown, and possibly non-existent, cause exceeds so-and-so, n being a definite specified number? And what can the word "chance" mean in that connection? Quantitative "chance" has significance only in relation to a family of events, and its value for a given event depends upon the characteristics of the family as well as upon that of the event itself. But as regards the uneliminated systematic errors, his observations define no family. He has nothing from which to compute a chance. All he can validly do is to express an opinion; and that opinion can validly relate only to certain theoretical considerations and to the magnitude of the errors that might have escaped his attention, not to any chance that his result might be in error by a given amount.

In every report, such an opinion of the limits within which the quaesitum is believed to lie, based solely on the work being reported, should be given. But in addition to that, previous measurements of the same quantity, when available, will usually be compared with those being reported, for one or more of the following purposes: supporting the author's value; setting other limits for the range within which the author thinks the quaesitum lies; deriving a general definitive value. But even in these cases only the same kind of opinion can be expressed, the number of absolute determinations that have been made of any given physical quantity being far too small to define a statistical family. (pp. 10 and 11.)

The experimenter's opinion must rest on evidence, if it is to have any weight. And the only evidence available comes from theory, the series of observations made in the course of the work, and the diligence with which errors were sought. These, and in particular the discordance of the observations and the diligence of the search, are what must be depended upon. Dependence on theory is weak, for the actual conditions never accord exactly with those assumed in the theoretical work.

He knows that it is impossible to avoid systematic errors, that even when he has done his best, his result is still haunted by the ghosts of such errors. His whole problem has been to seek such errors out, and to eliminate them when found; and he believes that in his long search any existing combination of them that would have produced an effect greater than the limit he sets would have been found. But he would be the first to admit that he may be wrong, that his result might be affected by a much larger error arising in such a way that, in spite of the many changes made in the course of the work, it remained essentially unchanged; but he thinks that contingency is highly unlikely. However, he is not entitled to that opinion unless he has carried out the indicated search, for in no other

way can a foundation be found on which to base an opinion.

In the absence of such a search, the worker can do no more than hope that all is going well. The fact that he sees no reason for suspecting the presence of an unknown systematic error is of no importance at all, no matter who the observer is. The really troublesome errors are exactly those that are not suspected. The suspected ones can usually be to some extent eliminated. (p. 12.)

Report

The work should be fully reported, so that the reader may know what was done, may have the means for forming an independent judgment of the work and for checking possible errors and omissions, and may have the worker's experience to build upon in case he himself should undertake a similar piece of work. The last is certainly a very important function of such a report, and should never be ignored.

The report should, of course, give a clear indication of the care with which search was made for sources of error, and of the thought that was given to it. Otherwise, one has no choice but to conclude either that no search was made, or that the author attached no special importance to it. In either case, the work is of little if any, objective value; its acceptance can rest only on authority, on subjective grounds.

Data should be reported as fully as may be. But in every series of observations some are erratic especially at the start. How should they be treated? Those that occur in the body of the work should certainly be reported as fully as if they were not erratic, and if the cause of the trouble is known, that should be explained.

Those that occur peculiarly at the beginning of the series, arising mainly from maladjustment and inexperience, furnish very valuable information regarding details of adjustment and manipulation that had escaped the foresight of the worker, and that might, therefore, readily escape the attention of the reader and of subsequent workers. In certain cases they give valuable information about unsuspected sources of error. For such reasons, they should never be completely omitted. They need not always be given in full, but they should be given to such an extent and in such detail as will show the reader what they were like and how they were related to the pertinent conditions, and should be accompanied by such explanatory text as will show him how they were regarded by the worker, and how he contrived to remove the disturbing conditions.

In brief, the report should give the reader a perfectly candid account of the work, with such descriptions and explanations as may be necessary to convey the worker's own understanding and interpretation of it. Anything short of that is unfair to the writer as well as to the reader. Every indication that significant information has been omitted reduces the reader's confidence in the work.

It is the unquestioned privilege of the worker to say where the boundary lies between preliminary or trial determinations, made primarily for studying and adjusting the apparatus and procedures, and those that were expected to be correct. But he should give good reasons for placing that boundary where he does; and those preliminary determinations should be reported to the extent already indicated.

Furthermore, it is scarcely fair, to anyone concerned, to describe a series of determinations as "preliminary," thus implying, in accordance with common usage, that they are open to question, that they are merely preparatory for something better, and then, later on, to include that same series in the list of good, acceptable determinations. To do so, both confuses the reader and suggests to him that the use of the adjective "preliminary" may have been merely a face-saving device intended to justify the ignoring of that series in case it should be found to disagree uncomfortably with later ones. (pp. 12 and 13.)

Miscellaneous

To say that an observer's results are influenced by his preconceived opinion does not in the least imply that those results were not obtained and published in entire good faith. It is merely a recognition of the fact that it seems more profitable to seek for error when a result seems to be erroneous, than when it seems to be approximately correct. Thus reasons are found for discarding or modifying results that do violence to the preconceived opinion, while those that accord with it go untested. An observer who thinks that he knows approximately what he should find labors under a severe handicap. His result is almost certain to err in such a direction as to approach the expected value.

The size of this unconsciously introduced error is, obviously, severely limited by the experimenter's data, by the spread of his values. The smaller the spread, the smaller, in general, will be this error. The size will be much affected also by the circumstances of the work, and by the strength of the bias. If the work is strictly exploratory, its primary

purpose being to find whether the procedure followed is at all workable, then only a low accuracy will be expected, and there will be no serious attempt to explain departures from the expected, even though the departures be great. Consequently, this error of bias may be entirely absent from such results. But if the worker is striving for accuracy, then departures from the expected will appear to him serious; and the stronger the bias, the more serious will they seem. He will seek to explain them; and that seeking will tend, in the manner already stated, to introduce an error. An error arising in this way will seldom be negligible, but in no case should one expect it to be great, the work being done in good faith. (p. 2.)

. . . published definitive values, with their accompanying limits of uncertainty, are not experimental data, but merely the authors' inferences from such data. Inferences are always subject to question; they may be criticized, reexamined, and revised at any time. (p. 3.)

. . . it is every author's duty to publish amply sufficient primary data and information to enable a reader to form a just and independent estimate of the confidence that may be placed in the inferences that the author has drawn therefrom. If he does not, he is false to both his reader and himself, and his inferences should carry little weight, no matter how great his reputation may be; . . . .

Indeed, values reported without such satisfactorily supporting evidence have no objective value whatever, no matter how accurate they may happen to be. They rest solely on the authority of the reporter, who is never infallible. (p. 3.)

FOREST IN THESE DAYS

Where the tall branch of maple swings,
A few young Chebecs test their wings
All unaware of what we mean
By atoms split, or radar screen.
With all our thought and human powers,
We cannot make the forest ours,
Though we may walk its dim trails over
And flush the partridge from their cover.
Yet we may feel some vague surprise
That vivid trees still point to skies.

DANIEL SMYTHE

Delanson, New York

SCIENCE ON THE MARCH

PROTEIN STRUCTURE PROJECT

THE empirical manner in which research in the protein field must proceed has long been a thorn in the side of biochemists. Besides requiring untold hours of cut-and-try experimentation, it leaves unanswered the fundamental questions on the nature of proteins.

Without proteins there is no life. Proteins exist only as part of and as products of life. Many agents producing disease owe their destructiveness to the proteins they contain. Yet, the structure of not one single protein is known!

In spite of the immense variety of proteins and their range from beneficial to harmful, they are known to have many common characteristics. For instance, they can all be broken down into a very few chemical substances, all of which are called amino acids; this is evidence that the underlying structure of all protein molecules is similar. There is a great deal of other evidence, all pointing in the same direction. However, the structure—the way atoms are arranged and fastened together—in even one kind of protein molecule is still unknown. If this underlying structure were known in the fundamental molecule of a protein, it would doubtless be possible to explain many of the ways that proteins behave and even to predict new things that proteins could do.

Among the things needing explanations are: What is the difference between a cooked and an uncooked egg? (That is: What happens to the proteins of which an egg is composed when they are heated?) Once an egg is cooked, why can it never be made into an uncooked egg? Why do viruses, which are composed of protein molecules swimming in some sort of liquid, sometimes cause diseases and sometimes not? Why does an antitoxin prevent a particular disease, but not others? What is the difference between cancerous and healthy living tissue? And there are, of course, many other questions one can ask about proteins. All these questions could be attacked with much greater confidence of success if one had a knowledge of the nature of even one protein's structure.

One may ask, "How can the structure of a molecule be found?" Many answers to this question have been discovered, and the most universally successful answer is, "By a study of the way x-rays are scattered from crystals consisting of these molecules." There is a huge background of experience and skill in successfully working out the structures of even quite complicated molecules by the x-ray scattering method. It is true that the structure of a protein molecule is much more complicated than that of any molecule attacked up to now, but the methods, experimental, mathematical, and theoretical, are at present powerful enough to give one confidence that a determined attempt to find the structure of a protein molecule can be successful.

The work involved is difficult and extensive; it is necessary to use the talents of several kinds of specialists and to resort to the most modern and complex computing machinery, as well as the best laboratory equipment; but the probability of success is very great.

With this in mind, there was formed in June, 1950, the Protein Structure Project of the Polytechnic Institute of Brooklyn, under the direction of Dr. David Harker, formerly head of the Crystallography Division of the General Electric Research Laboratory, Schenectady. It is sponsored by the Dean Langmuir Foundation, the Rockefeller Foundation, and the Damon Runyon Fund. The Institute contributes location and overhead; the International Business Machines Corporation provides calculating facilities.

This is a straight-line ten-year project with one objective—to determine the atomic positions within protein crystals. The principal tool in this program is a General Electric x-ray diffraction unit adapted to the extremely delicate type of analysis that must be employed on protein molecules. The molecular weight of a protein is 5000 or greater, which poses problems of unusual magnitude in crystallographic studies.

The central requirement of this diffraction apparatus is to permit setting crystal orientation to hundredths of a degree. Dr. Harker and his associates have added to the usual spectrogoniometer a unit tentatively termed an Eulerian cradle, which makes it possible not only to orient the protein crystal with the required accuracy, but also to minimize the exposure of the crystal to x-radiation and thus reduce the change produced in the sample itself by x-radiation.

The x-ray beam collimator is now adapted specifically for single crystal work, with background

reduced to an absolute minimum so as to provide as little interference as possible with the weak protein crystal-diffracted x-rays. The intensity of the diffracted radiation obtainable from protein crystals is only a thousandth of that from single crystals of calcite or quartz. A maximum count would be in that neighborhood of 500 x-ray quanta per second. With the new collimator, background has been reduced from 3 to 10 counts per second, at present, and may go lower as the apparatus is improved.

In the protein structure project, studies are made of samples which diffract at very small angles, compared with the materials encountered in ordinary analytical work. Diffraction angles will be of the order of 1 degree up to 60 degrees at the most, whereas the smallest angle at which iron crystals diffract is about 45 degrees. These requirements call for much higher resolution and control than are customary in other single crystal analysis.

A combination of crystallographic techniques is employed by the project. Photographic recording of diffracted x-ray intensities is used in certain phases, but for others, data are obtained with the Geiger counter spectrometer by counting directly the diffracted intensities. The aim is to get intensities accurate to 5 per cent, and in a shorter length of time than has been possible hitherto—shorter, that is, by from 5 to 10 times.

The effects of x-radiation on the sample, it is planned, will be eliminated by replacing the crystal as soon as such an effect can be detected.

Proteins are high polymers produced only by living matter. The simplest viruses are regarded as nothing more than very large protein molecules, with weights running into the millions. Each molecule is alive, for it eats and reproduces itself.

However, in this project, work has begun, of course, with the simplest structures, molecules measuring only around 30 to 50 angstrom units in diameter. The work now in progress consists of three main programs: (*a*) the preparation of protein crystals, (*b*) the obtaining of x-ray diffraction data, and (*c*) the conversion of these data into knowledge of protein structure.

These programs are now in the following stages: (*a*) Crystals of several allotropic forms of the enzyme ribonuclease have been grown large enough to be useful for x-ray diffraction experiments, and their diffraction patterns are being obtained.

(*b*) The data from which a description of protein molecular structure will be derived are the intensities and directions of the x-ray beams diffracted by protein crystals. These data can be recorded either photographically or by means of a Geiger-Müller counter. Devices for recording photographically the single-crystal diffraction data have been purchased and are now being used. Another device in which a counter collects the data has been assembled from some purchased parts and some parts constructed by the project's instrument maker. This device has been used to obtain data from some ribonuclease crystals given to the project by Professor I. Fankuchen and will soon be used on other crystals. In addition to these sets of data, lists of intensities have been borrowed from two other laboratories: on insulin from Dr. Dorothy Hodgkin of Oxford University and on hemoglobin from Dr. F. Perutz of Cambridge University. At the same time as the collection of data just mentioned, a device is being designed and constructed by means of which all the data from a crystal will be collected automatically and recorded on IBM punched cards. This device will not be in operation for at least a year, but should justify the effort being put on it in a very short time after it is first put into use.

(*c*) The data to which reference has been made so often are related to the crystal structure in a rather complicated, but definite mathematical way, so that the knowledge of the data is, in principle, a knowledge of the crystal's structure. At one point in the computation there is a difficulty, however, which is equivalent to deciding for each diffracted x-ray beam whether it should be positive or negative. There are ways of deciding this question, but they too involve a large amount of computation.

In consequence, it was felt necessary to obtain the cooperation of the International Business Machines Corporation before attempting to plan the project at all. This cooperation has been forthcoming in that the project personnel may work with the computing devices of the Watson Laboratory of IBM as much as is necessary to the success of the project. Use has been made of this privilege, and methods of computation suitable for crystal structure work on proteins have been devised and tested, using the data on insulin and hemoglobin borrowed from the English universities. These methods are now being tried out on the preliminary ribonuclease data recently obtained.

In addition to accomplishing the main objective, the knowledge of a protein structure, it is certain that many other very valuable pieces of information will be picked up during the course of the work. These may include results in protein chemistry, x-ray physics, mathematics, and equipment use and design. The sponsors of, and participants

in, this project count on a rich reward in scientific achievement, the results of which cannot fail to be of important and lasting value to all people.

Related work is being done at Harvard Medical School, the Massachusetts Institute of Technology, and California Institute of Technology, and at Cambridge University, Oxford University, and University of London, in England.

The permanent staff of the Protein Structure Project at Brooklyn Polytechnic Institute consists, in addition to Dr. Harker, of Dr. Murray Vernon King, chemist, who prepares and selects the protein crystals suitable for analysis; Dr. Thomas C. Furnas, Jr., physicist, in charge of designing apparatus for diffraction data; Dr. Beatrice S. Magdoff, physicist, who converts diffraction data into useful structural information; William G. Weber, precision instrument maker, who constructs and maintains the apparatus and laboratory equipment; and Mrs. Harker, secretary and librarian. In addition to the permanent staff, other scientists will spend periods of time working in the laboratories of the project; Dr. V. Luggati is the first of these.

The laboratories occupy 1200 square feet, divided into an x-ray diffraction laboratory with connecting photographic darkroom, chemical laboratory, three office rooms, and a machine shop.

DAVID GOODMAN

Milwaukee, Wisconsin

BOOK REVIEWS

WHAT MAKES AN INSECT TICK

Insect Physiology. Kenneth D. Roeder, Ed. xiv + 1100 pp. Illus. $15.00. Wiley, New York; Chapman & Hall, London. 1953.

IT is evident why book reviews of this fine text have been slow in appearing, for it is not recommended for light fast reading, even by the specialist. On the other hand, applied perusal quickly underlines the fact that insect physiology has "come of age," if the specializations evidenced in the respective treatments of different phases of the subject by fifteen authorities, two of them Australian and the others American, are adequate criteria. At least it would be as difficult for any one person to marshal the facts with the mastery accomplished sectionally in this technical treatise, as for another one to review it as adequately.

The method of presentation indicates that it is not intended as a teaching textbook. On the other hand, it will unquestionably prove to be an indispensable reference work both for teaching and research. The 107 pages of bibliography attest the thorough documentation of subjects ranging from considerations of the integument and cuticle, circulation, respiration, nutrition, excretion, behavior, and embryology—through 32 chapters to hormones in metamorphosis.

That insects are peculiarly well suited to certain types of physiological experimentation is repeatedly shown, and the familiar *Drosophila* flies again play one of the leading roles. Many comparisons with the more advanced knowledge in equivalent vertebrate fields add to the value of the work. For example, chemoreception is considered as being basically the same in human beings and in insects, which makes the latter ideal experimental subjects. The editor's prefatory remark that the insect physiologist shares "with entomologists an interest in insects per se" is well taken, and emphasizes all the more the major omission of sections on the physiology of the reproductive and light-production systems, of which there is no equivalent lack of literature. One would also have liked to find more reference to physiological advances in the fields of symbiosis, parasitism and parasitic acarina, and relationships to

disease agents, among others. Though there is considerable discussion of optic mechanisms and developments, one looks in vain for any further discussion of the functioning of the previously speculative, so-called "night-eyes" of certain male Diptera, with their upper areas of much enlarged facets. But there are limitations to what can be put into one book, as the reviewer can attest (this volume already weighs just four pounds)—not to mention the technical dictionary often needed for a not inconsiderable amount of terminology unfamiliar in my own limited background. These are all evidence of an advancing discipline and not a criticism.

The book abounds in interesting facts, such as the almost incredible record of the alternation of flight muscle movement in flies, as high as 1046 per second—far beyond the observed performance of stimulated frog muscle and the humming bird among higher vertebrates—and the ingenious use of surgical devices in demonstrating ontogenetic tissue growth and differentiation by which the subsidiary science of insect endocrinology was launched. It was startling to learn that in some insects the compound eyes are developed by invagination of the epidermis of the larval head capsule, whereas in others they develop from embryonic pharyngeal tissue. Parts of chapters on insect behavior and social patterns, while fascinating, would seem more appropriate in some ecological treatise. On the other hand, for other specialized entomological groups such as insect toxicologists, this book will quickly become one of their most important reference books. The text is remarkably free of typographical errors. A useful innovation is the page-use citation for each of the bibliographic references. Furthermore, some chapters have helpful summaries or conclusions.

This reviewer, as an "outsider," feels well repaid for having read through this comprehensive treatise with a view to future reference. Any shortcomings it has will undoubtedly be rectified when revised, or will "point the finger" to the many important lacunae still to be investigated—and these indeed have often been called to attention in the present text. The book is a worthy testimonial to the maturation of insect physiology, although in this early development of the science it still is far from answering all the complicated questions on what makes an insect tick.

CORNELIUS B. PHILIP
National Microbiological Institute
Rocky Mountain Laboratory
Hamilton, Montana

CELLULOSE AND INDUSTRY

Cellulose, the Chemical That Grows. Williams Haynes. 386 pp. Illus. $4.00. Doubleday, Garden City, New York. 1953.

CELLULOSE, in both bulk and value, is a major raw material of the American chemical industry. This book describes the historical development and present condition of each of the important chemical industries that are based on cellulose, including the explosives, lacquer, film, plastics, and synthetic textile industries. The author is personally acquainted with many of the pioneers in the development of these industries and this fact gives the book the authenticity and intimate detail of an eyewitness account. The historical style that mingles in a dramatic fashion important laboratory discoveries with patent law suits, missed opportunities with astonishing commercial successes, makes fascinating reading for a nontechnical audience. The book also includes a chronology of cellulose technology, a technical glossary, and appendices of statistical data. The 45 illustrations are beautifully reproduced and informative. The only faults are lack of bibliography and a rather weak index. The book is heartily recommended to everyone from high school students to salesmen and financiers who may be interested in the development of an important segment of American industry. It is seldom that one finds a more carefully written, informative, and thoroughly interesting book.

MARK H. ADAMS
Department of Microbiology
New York University Bellevue Medical Center

BACTERIOLOGY FOR NON-SCIENTISTS

Microbiology and Human Progress. Madeleine Parker Grant. xvii + 718 pp. Illus. $6.75. Reinhart, New York. 1953.

THIS college textbook will serve primarily the undergraduate or graduate student whose major interests lie outside science. Many such students must take one course in a natural or physical science and are antagonized by rather than attracted to microbiology when forced to compete with science majors and to use a technical textbook that presupposes background in biology and chemistry. Dr. Grant's recognition of the need for a textbook for non-scientists has resulted in frequent use of descriptive material relating microbiological observations with phenomena associated with higher forms of life, interesting historical backgrounds to many microbiological discoveries, excellent illustrations, simple appropriate diagrams, and several medical case histories. The book has a good conversational tone and includes much of the philosophy of the author.

The book is *not* recommended for the use of students majoring in chemistry, biology, microbiology, bacteriology, or the various medical fields. Although the organization of all the material is fairly good (especially the regional medical grouping of diseases), the treatment of many subjects is nontechnical and often unbalanced. Science students should be given much more material on cell physiology, for example, than can be included in a textbook of this type. Also, of the 363 pages devoted to applied microbiological fields, only 14% deal with "microbic friends of man" whereas 86% are concerned with "microbic foes of man." Despite the inclusion of the word "microbiology" in

the title, the great majority of the general and applied sections deal specifically with bacteria, rickettsia, and viruses. Algae, yeasts and molds, and protozoa are discussed in only 18 pages of the general section; in the medical section, the pathogenic yeasts, molds, and Mastigophora are omitted completely. The space devoted to amebic dysentery in the medical section is only one-sixteenth the space devoted to typhoid fever although the author states that in the United States in 1949 twice as many cases of amebic dysentery as typhoid fever were reported. Important present-day bacterial infectious diseases that are not discussed include plague and cholera; others of lesser importance that are omitted include tularemia, psittacosis, spirochetal diseases other than syphilis, and leprosy. Infectious diseases of plants are not mentioned.

The chapter on chemotherapeutic agents was apparently written in 1947 (based on the contents and references) but nevertheless contains a considerable amount of useful information. Of all the chapters, approximately half contain some references to work published between 1949 and 1952. The index comprises only 15 pages which is unusually brief for a lengthy textbook. Perhaps the brevity of the index reflects the fact that the size of the book has been inflated by the large number of illustrations and by the large amount of nontechnical material of a philosophical, historical, or social nature. Minor criticisms include the use of the adjective "exotic" to describe rickettsia and viruses and to describe the inorganic nutrilites required by autotrophic bacteria; also, in the general section on molds the statement is made that sprue is a fungal infection.

None of the above criticisms will annoy the non-science student and instructors of courses in bacteriology designed for such students should, when selecting a text, seriously consider this book.

EUGENE D. WEINBERG

Department of Bacteriology
Indiana University

RADIOCHEMISTRY

Radioisotopes in Industry. John R. Bradford, Ed. vii + 309 pp. Illus. $8.00. Reinhold, New York. 1953.

THIS book consists of a compilation of papers presented at a symposium under the title, *Radioisotopes in Industry,* presented in cooperation with the Atomic Energy Commission at Case Institute of Technology in April, 1951. A wealth of valuable and interesting information is made available within the covers of this one book.

It contains fourteen chapters, each written by a different author. The chapter headings indicate clearly the scope of information presented: Radioisotopes, A New Industry; Radioisotopes for Industry; Fundamentals of Radiochemistry; Radiation Protection; Radioisotopes in Physical and Chemical Research; Applications of Radioisotope Techniques; Radiochemical Laboratories, Design of Laboratories for Safe Use of Radioisotopes; Production and Separation of Radioisotopes; Tracer Experiments; Instrumentation in the Radiochemical Laboratory; Decontamination and the Disposal of Radioactive Wastes; Industrial Uses of Radioactive Fission Products; Distribution of Radioisotopes by the Atomic Energy Commission.

The style of writing varies from chapter to chapter, as should be expected in a book of this type. All authors are authorities in the field and the presentations are all clear, informative, and interesting. Readers who are not acquainted with the field will find the book very readable because no assumptions have been made concerning prior knowledge of radioisotopes. Also, the expert in radiochemistry as well as those whose interests are primarily in industry will read this book with considerable interest and profit.

Each chapter is written as a complete unit. This means that there is some repetition from chapter to chapter, yet it also makes it possible for the reader to select only those chapters in which he is especially interested if he so desires. The headings throughout the chapters are well arranged. Each chapter concludes with a list of general references for supplementary readings.

The reviewer noted throughout a very proper emphasis on the facts that the industrial uses of radioisotopes have already become very important and yet that the surface of tremendous possibilities has hardly been scratched. This book is to be highly recommended, both to experts in the field and to those members of the general public who are interested in knowing more about the industrial age in which we live.

ROBERT D. FISCHER

Department of Chemistry
Indiana University

MEN OF NERVES

The Founders of Neurology. Webb Haymaker, Ed. 479 pp. Illus. Thomas, Springfield, Ill. 1953.

THIS collection of biographical vignettes, describing the life and work of 133 founders of neurology, is a new departure in the writing of scientific history. The volume brings together brief but intimate accounts of outstanding investigators in the neurological field, including anatomists, physiologists, pathologists, clinicians, and surgeons. The biographies are written by eighty-four authors, mainly Americans, who have usually been students, friends, or colleagues. The book was conceived by a group of members of the American Neurological Association at its meeting at Atlantic City in June, 1948. It was to have been presented to the Fourth International Neurological Congress at Paris in 1949, but has only now come from press. The volume is notable in bringing together photographs of all of the workers discussed, an achievement that adds much to the sense of intimate insight into their lives.

Skillful editing has produced a style of presentation that runs consistently through the whole volume. In

ach biography the main scientific contributions are outned. Relationships and associations between workers re indicated so that a three-dimensional picture of eurological activity, in space and time, builds up in he reader's mind. Of the men studied all but sixteen ere alive in 1880, and the account covers mainly the entury from 1830 to 1930. The biographies contain any stories and anecdotes, illuminating the scientific hilosophy, or personal traits, of members of the group. The origins of terms and phrases, now widely used in the field, are frequently made clear. The history of classical controversies and discoveries is outlined.

This book will be cherished by all those who now investigate or teach in the field. It will be an inspiration and challenge to younger men hoping to enter it.

WILLIAM R. AMBERSON

Department of Physiology
University of Maryland Medical School
Baltimore, Maryland

Books Reviewed In SCIENCE

July 10

Advances in Cancer Research. Vol. I. Jesse P. Greenstein and Alexander Haddow, Eds. New York: Academic Press, 1953. 590 pp. Illus. $12.00.
Reviewed by Michael B. Shimkin.

Science and Humanism: Physics in Our Time. Erwin Schrödinger. New York: Cambridge Univ. Press, 1952. 68 pp. $1.75.
Reviewed by Karl Lark-Horovitz.

Record of the Rocks: The Geological Story of Eastern North America. Horace G. Richards. New York: Ronald, 1953. 413 pp. Illus. $6.00.
Reviewed by George W. White.

July 17

Plant Anatomy. Katherine Esau. New York: Wiley; London: Chapman & Hall, 1953. 735 pp. Illus. + plates, $9.00.
Reviewed by Richard H. Goodwin.

Clinical Allergy. French K. Hansel. St. Louis, Mo.: Mosby, 1953. 1005 pp. Illus. $17.50
Reviewed by W. C. Spain.

Zoogeography of the Sea. Sven Ekman; trans. from Swedish by Elizabeth Palmer. London: Sidgwick and Jackson, 1953. (Distributed by Macmillan, New York.) 417 pp. Illus. $6.50.
Reviewed by Gordon A. Riley.

The Permeability of Natural Membranes. Reissue. Hugh Davison and J. F. Danielli. New York: Cambridge Univ. Press, 1952. 365 pp. Illus. $6.00.
Reviewed by L. J. Mullins.

July 24

Methods and Principles of Systematic Zoology. Ernst Mayr, E. Gorton Linsley, and Robert L. Usinger. New York–London: McGraw-Hill, 1953. 328 pp. Illus. $6.00.
Reviewed by G. G. Simpson.

Fatigue and Fracture of Metals. William M. Murray, Ed. Cambridge, Mass.: Technology Press of the Massachusetts Institute of Technology; New York: Wiley, 1952. 313 pp. Illus. $6.00.
Reviewed by A. G. Guy.

The Theory of Homogeneous Turbulence. G. K. Batchelor. New York: Cambridge Univ. Press, 1953. 197 pp. Illus. $5.00.
Reviewed by R. C. Binder.

July 31

Flying Saucers. Donald H. Menzel. Cambridge, Mass.: Harvard Univ. Press, 1953. 319 pp. Illus. $4.75.
Reviewed by C. C. Wylie.

Physiological Foundations of Neurology and Psychiatry. Ernst Gellhorn. Minneapolis: Univ. Minnesota Press, 1953. 556 pp. Illus. $8.50.
Reviewed by Robert G. Grenell.

ASSOCIATION AFFAIRS

THE BOSTON MEETINGS OF THE ASSOCIATION: A BIT OF BACKGROUND

THE 120th Meeting of the AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, the annual meeting for the year 1953, is also, officially, the Seventh Boston Meeting. The AAAS was conceived in Boston 106 years ago and, in some respects, this can be considered the ninth meeting in Boston and the tenth on the banks of the Charles—since the precursor of the Association met twice in that city and the young AAAS held its second meeting in Cambridge, in 1849. This year's gathering of scientists, industrial leaders, administrators, educators, engineers, and other science-minded professional people from all over the continent will come together for a common purpose suggested by the theme: "Scientific Resources for Freedom." In the final week of the year, it will be time once more to take stock both of current scientific research and of the problems that confront all scientists. Particular attention will be given to the nation's resources of scientific men, materials, and methods.

In meeting in Boston again, the Association is returning to the city where its founding was planned and authorized, on September 24, 1847. It was at the eighth and terminal meeting of the Association of American Geologists and Naturalists on this date, more than a century ago, that the decision was made to reorganize the society as an enlarged American Association for the Promotion of Science. The chairman of the society at that time was William Barton Rogers (1804–1882), professor of geology and natural history in the University of Virginia, who, later, was to select Boston in which to found the Massachusetts Institute of Technology and to serve as its first president. When the new organization, renamed the AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, met in Philadelphia, September 20, 1848, a special resolution was passed that Professor Rogers, last president of the AAGN, henceforth should be recognized as the first president of the AAAS and, in fact, he presided until his elected successor, William C. Redfield of New York, took office.

The AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE has other links with Boston and the Commonwealth of Massachusetts, from which, in 1874, it received its charter of incorporation. In 1837, John Collins Warren of Boston read a paper before the British Association for the Advancement of Science and was so impressed with the value of one large meeting devoted to all the sciences that, upon his return the following year, he began the active promotion of a parallel organization in America. He, too, was present at the September 1847 meeting when the AAGN was reorganized. Since the British Association for the Advancement of Science, founded in 1831, is the prototype of the AAAS and of all similar associations for the advancement of science subsequently established throughout the world, it is particularly appropriate that a recent past president of the British Association, Dr. A. V. Hill, will deliver an address at this Seventh Boston Meeting.

The official First Boston Meeting of the AAAS, held in August, 1880, at Massachusetts Institute of Technology, then located between Copley Square and the Public Gardens, was an occasion, scientific and social, long to be remembered. Lewis H. Morgan, the renowned anthropologist, was president of the Association, which then had 1,555 members. The address of the retiring president, George F. Barker, an outstanding chemist of the period, was on "Some Modern Aspects of the Life-Question." There were 979 registrants from more than 30 states, Canada, England, and Cuba and 276 papers were read. As first past president of the Association and also as first president and founder of M.I.T., the host institution, it was eminently fitting that William Barton Rogers, though now 76 years in age, should serve as General Chairman and deliver an address of welcome. The local "Committee at Large" included Charles Francis Adams, Charles W. Eliot, Ralph Waldo Emerson, Asa Gray, Oliver Wendell Holmes, Henry W. Longfellow, Francis Parkman, and Josiah Quincy—to name but a few. Samuel H. Scudder and Edward Burgess were secretaries.

In this more leisurely, less complicated period, M.I.T. served complimentary lunches daily. The President and Fellows of Harvard University entertained the entire attendance at dinner in Memorial Hall. There were receptions, notably those by President and Mrs. Rogers, Mr. and Mrs. Alexander Graham Bell, and Mr. and Mrs. S. Endicott Peabody, and many open houses—including those sponsored by the Athenaeum, the Boston Society of Natural History, the Massachusetts Historical Society, and the Massachusetts Horticultural Society—and the City of Boston provided an excursion boat trip down the harbor complete with a collation. To facilitate reaching the sessions from the downtown hotels, "those cars passing by the Institute [were] designated by a white flag, with

the letters A.A.A.S. . . . " The Western Union Telegraph Company and the American Bell Telephone Company transmitted the messages of the delegates gratis, the Post Office arranged to be open on Sunday morning, and the railroads not only had special rates for general convention travel but operated free trains to the White Mountains.

The Fiftieth Anniversary of the Association was celebrated at the Second Boston Meeting of August, 1897, with another distinguished anthropologist, Frederic W. Putnam, who had served the AAAS as permanent secretary for 25 years, now the president. M.I.T. again was host institution. The Copley Square Hotel was AAAS headquarters —with single rooms at $1.00 to $2.50. The address of Wolcott Gibbs, retiring president, and one of five surviving founders of the Association, was "On Some Points in Theoretical Chemistry." The Honorary President, Governor Roger Wolcott, took a personal interest in this meeting and delivered an excellent address. The papers read totaled 443 and the registration was 903.

By the time of the Third Boston Meeting, in 1909, once more on the former campus of M.I.T., the Association had changed its time of meeting from summer to the last week of December (primarily, because of the development of summer sessions on campuses), and the pattern of participation by a large number of scientific societies was well established. David Starr Jordan, eminent zoologist and university president, was president of the Association; the retiring presidential address, "A Geologic Forecast of the Future Opportunities of Our Race," was given by Thomas C. Chamberlin. Harry W. Tyler was General Chairman. There were 1,140 registrants, making this the largest AAAS meeting up to that time. Among the 404 papers read was "The Chemist's Place in Industry" by Arthur D. Little, founder of the firm which bears his name today. A national Bureau of Mines was recommended by the AAAS.

The Fourth Boston Meeting of December, 1922, with the celebrated Canadian anatomist, J. Playfair McMurrich, as president, was held principally on the new campus of M.I.T. though, as on previous occasions, there were events at Harvard University in Cambridge. The address of retiring president Eliakim H. Moore was "What Is a Number System?" Professor Samuel C. Prescott of M.I.T. was General Chairman. The Somerset was AAAS headquarters hotel. The exhibits, arranged for by a committee headed by Professor Robert P. Bigelow, for the first time included a number installed by commercial exhibitors. It is gratifying to note that some of these pioneer exhibitors not only are still in business but will participate in this year's Exposition. The first of the annual addresses of the Society of the Sigma Xi at AAAS meetings was given by President Livingston Farrand of Cornell University on "The Nation and Its Health." The papers read totaled 1,019 and the registration was 2,339.

All local institutions of higher learning were hosts of the Fifth Boston Meeting of December, 1933. Sessions were held, principally, at Harvard, M.I.T., and at the Hotel Statler, AAAS headquarters. The exhibits, now the responsibility of a staff member, were in Harvard's Memorial Hall and, in number, exceeded those of all previous Expositions. It was a large and successful meeting despite the extremely low temperatures experienced by the entire East during this exceptional winter. The famous astronomer, Henry Norris Russell, was president of the Association, and presided at the address of retiring president, John Jacob Abel, eminent pharmacologist, on "Poisons and Disease." Again, Samuel C. Prescott served as General Chairman; A. Lawrence Lowell was Honorary Chairman. A much appreciated event was a complimentary testimonial concert given by the Boston Symphony Orchestra with Dr. Serge Koussevitsky conducting. The eleventh winner of the AAAS Thousand Dollar Prize was Reuben L. Kahn for the paper, "Tissue Reactions in Immunity," in the program of Section N. About 1,500 papers were read and there were 2,351 registrants, as usual from nearly every state and Canadian province.

Though, again, all local institutions were hosts of the Association, the Sixth Boston Meeting of December, 1946, was characterized by a much more intensive use of downtown hotels for session rooms. The Annual Science Exposition was located in the Cadet Armory near the Hotel Statler, AAAS headquarters. President of the Association was James B. Conant; the retiring president, Charles F. Kettering, gave his address, "A Look at the Future of Science," in Symphony Hall. David M. Little of Harvard University was General Chairman. The twentieth winner of the AAAS Thousand Dollar Prize was shared equally by T. M. Sonneborn, Ruth V. Dippell, and Winifred Jacobson for several papers on the mechanism of heredity in *Paramecium*, read before the American Society of Zoologists; and by Quentin M. Geiman and Ralph W. McKee for "Cultural Studies on the Nutrition of Malarial Parasites," read before the American Society of Parasitologists. A total of 2,736 persons registered and 1,332 papers were read. The first

AAAS-George Westinghouse Science Writing Award was won by James Graham Chesnutt of the San Francisco *Call-Bulletin* for a story on a bubonic plague preventive.

In summary, the records of all previous meetings in Boston do not fail to mention the warm spirit of hospitality and interest in the Association and its work shown by the people of this cultural center. The group of cities and suburban communities which comprise the Boston Metropolitan Area—now with a population of two and one-half millions—has one of the country's greatest concentrations of institutions of higher learning, and of libraries, museums, and scientific laboratories. New England is compact and New York is nearby, so that local and regional attendance added to the several thousand persons who will come from all parts of the continent to attend the programs of the Association's 18 sections and subsections, and the national meetings of the zoologists, geneticists, science teachers, meteorologists, the History of Science Society, and others, may make the Seventh Boston Meeting the second largest in the annals of the Association. In all, in national and regional meetings and cosponsored sessions, some 57 organizations will participate. With sessions for contributed papers, symposia, distinguished evening addresses, and a growing number of conferences, the Seventh Boston Meeting will be one of the most significant annual conventions in the long history of the Association. Of the 15 past presidents of the Association now living, five are residents of New England. It is hoped that they—Karl T. Compton, James B. Conant, Harlow Shapley, Edmund W. Sinnott, and Kirtley F. Mather*—and the others will be able to attend this year's meeting.

With its many historical landmarks, Boston itself is worth a visit at any time. Indeed, the "Points of Interest" are too numerous to describe in this year's General Program-Directory. Instead, each registrant will receive a complimentary printed handbook at the Main Registration–Information Center in the Mechanics Building. Founded in 1630, since colonial times, Boston has been a seaport, the banking and commercial metropolis of New England, and a great industrial center. In recent years, this city has become noted as the site of new and important developments in chemistry, electronics, and nuclear physics. Many of these new "scientific resources for freedom" will be on display in the 160-booth Annual Exposition of Science and Industry in Mechanics Building. It is particularly fitting that the General Chairman of this year's 120th AAAS meeting is Earl P. Stevenson, president of Arthur D. Little, Inc. Not only is he the leader of a company that has pioneered in the organized applications of science, but he is active in a number of national scientific organizations. His appointments —the many persons who are working to make the Seventh Boston Meeting an unqualified success—will be listed later. The fruits of their contributions of time and thought will be apparent to those who attend this year's meeting.

The AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE continues to grow, both in personal membership and in affiliated professional societies and academies of science. In just the seven years since the Sixth Boston Meeting, the AAAS, now with a membership of approximately 50,000, has experienced a net gain of more than 21,000 members. In 1946 there were 200 affiliates and associates; at this time, the number of affiliated and associated organizations is nearly 250. The Association's capacity for service to science, to scientists, and to society has been correspondingly enhanced. Fundamentally, the Association *is* its membership. Those who attend the Seventh Boston Meeting will do much to help chart its future course.

RAYMOND L. TAYLOR
Associate Administrative Secretary, AAAS

* The other ten living past presidents are Liberty Hyde Bailey, Robert A. Millikan, Henry Norris Russell, Albert F. Blakeslee, Irving Langmuir, Arthur H. Compton, Anton J. Carlson, Charles F. Kettering, Elvin C. Stakman, and Roger Adams.